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PHILOSOPHICAL
TRANSACTIONS

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PHI-

PHILOSOPHICAL TRANSACTIONS.

XIII. *An Account of the Tabaſheer. In a Letter from* Patrick Ruſſell, *M. D. F. R. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read March 11, 1790.

SIR,

SHOULD the following remarks on the Tabaſheer, a medicine in high repute in many parts of the Eaſt, appear deſerving a place in the Tranſactions, you will do me the honour to preſent them, together with the accompanying ſpecimens, to the Society.

This drug was, I believe, firſt introduced to the knowledge of the weſtern world through the works of the Arabian phy-

ficians, all of whom mention it as an important article in their Materia Medica; and, from what I could obſerve in Syria, it ſtill continues to be in much more general uſe in Turkey than in this part of India.

To the Arabs and Turks it is known under the name of Tabaſheer only; at leaſt I never heard any other name given to it in Syria, nor even at Conſtantinople; under that name alſo it is mentioned by the Arabian writers.

In this country, beſides that of Tabaſheer, which they had from the Perſians, it is known under ſeveral other names.

In the Gentoo language it is called *Vedroo-paloo*, Bamboo-milk; in the Malabar, *Mungel Upoo*, Salt of Bamboo; and in the Warriar, *Vedroo Carpooram*, Bamboo Camphor.

Don GARZIA DALL' HORTO has long ago expoſed a dangerous error, common to the old tranſlators of the Arabian writers, reſpecting this drug. In the Latin verſions of Rhazis and Avicenna, Tabaſheer is conſtantly rendered Spodium; and this interpretation has been adopted by moſt of the ſubſequent tranſlators of other Arabian medical writers.

The late Mr. CHANNING, when engaged in the tranſlation of RHAZIS on the Small-pox, applied to me, then in Syria, for ſuch information as I might be able to collect on the ſubject of Tabaſheer at Aleppo. I accordingly tranſmitted to him various ſpecimens of the drug, together with ſeveral extracts relative to it, from books found in the Aleppo libraries. Some of thoſe ſpecimens differed conſiderably from thoſe now laid before the Society; and, from what I have had occaſion to obſerve during my reſidence here, I am convinced, much of the drug commonly vended in Turkey is fictitious or adulterated.

Having none of the Arabian medical writers at hand, I cannot pretend to ſtrict accuracy; but, to the beſt of my recol-

lection, they generally agree in the Tabaſheer being a production of the Indian reed; more eſpecially of ſuch as have ſuffered from fire, kindled by the friction of the reeds one againſt the other; an accident ſuppoſed to happen frequently in the dry ſeaſon, among the hills, where the bamboo forms vaſt and impenetrable thickets.

Several of the mountaineers, with whom I have converſed on the ſubject, affirm, that the bamboo is not the only tree ſubject to accidental ignition by friction, and named one or two other trees liable to the ſame accident; but added, they never looked for Tabaſheer in the half-burnt fragments of the bamboo, though they doubted not it might ſometimes be found there as well as in others.

The genuine Tabaſheer is undoubtedly a production of the Arundo Bambos of LINNÆUS, the Ily of the Hortus Malabaricus, and the *Arundo Indica arborea maxima, cortice ſpinoſo*, of HERMAN. It is no leſs certain, that fire is not a neceſſary agent in its production, whether the conflagrations in the mountains juſt now mentioned be reckoned fabulous or not.

The bamboo in which the Tabaſheer is found is vulgarly called the Female Bamboo, and is diſtinguiſhed by the largeneſs of its cavity from the male, employed for ſpears or lances. They are ſaid to be ſeparate trees; but this fact I have not had it in my power to aſcertain.

Of the ſeven pieces of bamboo which accompany this Paper, four are from the mountains in the vicinity of Vellore, and three from a place twenty miles from hence. The former were perfectly green on their arrival at Madras; and the others were ſelected from a large parcel, which were green alſo when they came to my hands. Theſe were all ſelected on a con-

jecture of their containing Tabaſheer, from a certain rattling perceived upon ſhaking the bamboo, as if ſmall ſtones were contained in the cavity.

This, by the natives, is conſidered as an indication of Tabaſheer being contained in one or more joints of the bamboo, and they are ſeldom diſappointed; but it does not always follow, that there is no Tabaſheer where a rattling is not perceptible; for, upon ſplitting a number of reeds, it was ſometimes remarked, that where the quantity of the drug was inconſiderable, it was found adhering ſo cloſely to the ſides of the cavity, as to prevent any rattling from being perceived upon ſhaking. In general, however, the rule of the natives for chooſing the bamboos proved a good one.

In the month of April, one of the bamboos, conſiſting of ſix joints, received from Vellore, being cautiouſly ſplit, each joint was examined ſeparately. In two of them no veſtige of the drug was diſcovered; each of the others contained ſome, but in various quantity; the whole collected amounted to about twenty-ſeven grains.

The quality alſo was various. The particles reckoned of the firſt quality were of a bluiſh white colour, reſembling ſmall fragments of ſhells; they were harder than the others, but might eaſily be crumbled between the fingers into a gritty powder, and when applied to the tongue and palate had a ſlight ſaline teſtaceous taſte: they did not exceed in weight four grains. The reſt were of a cineritious colour, rough on the ſurface, and more friable; and intermixed with theſe were ſome larger, light, ſpongy particles, ſomewhat reſembling pumice-ſtones. It is probable, that the Arabs, from theſe appearances of the drug, were led into the opinion already mentioned of its production.

The

The two middle joints were of a pure white colour within, and lined with a thin film; it was in theſe chiefly the Tabaſheer was found. The others, particularly the two upper joints, were diſcoloured within, and in ſome parts of the cavity was found a blackiſh ſubſtance in grains or in powder adhering to the ſides, the film being there obliterated. In two or three of the joints, a ſmall round hole was found at top and bottom, which ſeemed to have been perforated by ſome inſect.

In the month of July, forty-three green bamboos, each conſiſting of five or ſix joints, were brought from the hills fifty miles diſtant from hence. Six, appearing to contain more Tabaſheer than the others, were ſet apart; the remaining thirty-ſeven were ſplit and examined in the manner beforementioned. The reſult was as follows.

In nine out of the thirty-ſeven there were no veſtiges of Tabaſheer. In twenty-eight ſome were found in one, two, or three joints of each; but never in more than three joints of the ſame bamboo. The quantity varied, but in all was inconſiderable; and the empty joints were ſometimes contiguous, ſometimes interrupted, indifferently.

The drug conſiſts of very diſſimilar particles at firſt when taken from the bamboo, as will appear in looking into the ſmall ſpecimen, N° 1.; which, having collected myſelf, I am certain has undergone no adulteration.

The whiter, ſmooth, harder particles, when not looſe together with the others in the cavity, were moſtly found adhering to the ſeptum that divides the joints, and to the ſides contiguous; but never to the ſides about the middle of the joints; and it may be remarked, that, inſtead of being chiefly found at the lower extremity of the joint, as might be expected from

the juice ſettling there, they were found adherent indifferently to either extremity, and ſometimes to both. In this ſituation they formed a ſmooth lining, ſomewhat reſembling poliſhed ſtucco, which uſually was cracked in ſeveral places, and might eaſily be detached with a blunt knife.

In ſome joints the Tabaſheer was found thus collected at one or both extremities only, and in ſuch no rattling was perceived upon ſhaking the bamboo; but generally, while ſome adhered to the extremities of the joint, other detached pieces were intermixed with the coarſer looſe particles in the cavity.

The quantity found in each bamboo was very inconſiderable; the produce of the whole twenty-eight reeds, from five to ſeven feet long, not much exceeding two drams.

It is remarked by GARZIUS, that the Tabaſheer is not found in all bamboos, nor in all the branches indiſcriminately; but only in thoſe growing about Biſnagur, Batecala, and one part of the Malabar Coaſt.

From the inconſiderable quantity procured from twenty-eight bamboos, it ſeems very probable, that, though not abſolutely confined to certain regions, it may be produced in greater abundance in ſome ſoils than in others; but that, in all regions where the bamboo grows favourably, ſome proportion of the drug will be found, however it may vary in quality or quantity.

RUMPHIUS on this ſubject refers to GARZIUS, candidly acknowledging, that he had not himſelf had opportunities of making particular enquiry. I expect anſwers from CEYLON to ſome queries ſent thither ſome time ago; and, in reſpect to Biſnagur, have been lately informed in a letter from Hydrabad, from a medical gentleman attending the preſent embaſſy to the Nizam, "That though Tabaſheer be in great requeſt at Hy-

"drabad,

" drabad, and bears a high price, it is never brought thither " from Biſnagur; that ſome of what is found in the Bazars is " brought from the Atcour paſs in Canoul, and ſome from " Emnabad at the diſtance of about eighty miles to the N.W.; " but that the greateſt part comes from Maſulipatam.

" That there are two ſorts ſold in the Bazars; one at the " rate of a rupee a dram; the other, of inferior quality, at " half the price; but that this is ſaid to be chiefly compoſed " of burnt teeth and bones.

" That he was informed by a Perſee, who had been in " Bengal, that the Tabaſheer was produced in great quantities " at Sylhet, where it ſold by the pound from one rupee to one " and a half, and formed a conſiderable article of trade from " Bengal to Perſia and Arabia."

N° 3. is a ſpecimen of the prime ſort from Hydrabad. It differs materially from the others, not only in its ſuperior whiteneſs, and the being leſs mixed with impure particles; but in the being much harder than the pureſt particles of my ſpecimens, much heavier, and hardly in any degree friable to the finger.

Submitting the ſpecimens to examination, I refrain from experiments on them which may more ſucceſsfully be made in England, and ſhall proceed to offer a few obſervations on the juice of the recent bamboo ſuppoſed to form the Tabaſheer.

RUMPHIUS remarks in Amboina, " Juniores arundines ple- " rumque in inferioribus ſuis nodis ſemi-repletæ utcunque ſunt " lympida aqua potabili, quæ hiſce in terris ſenſim evaneſcit, " in aliis vero regionibus exſiccatur in ſubſtantiam albam et " calceam, quæ Tabaxir vocatur."

GARZIUS gives an account ſomewhat different from this. I am obliged to cite from an Italian tranſlation. " Fra tutti

" gli

" gli intermezzi de' nodi, ſi genera un certo liquore dolce
" e groſſo, e ridotto in guiſa di farina d' amido, e della iſteſſa
" bianchezza, et alle volte ſe ne genera aſſai, alle volte poco,
" ma non tutte le canne, nè meno tutti i rami generano tale
" humore. . . . . Queſto liquore dopo d' eſſere appreſo,
" moſtra d' eſſere di color nero, over cinericcio, e non perciò
" é tenuto per triſto, imperoche queſto avviene, ò perche ſia
" troppo humido, ò perche ſia ſtato lungo tempo nel legno rin-
" chiuſo, ſi come s' hanno penſato alcuni: concioſia che in molti
" rami, che non ſono ſtati toccati dal fuoco, intravenga queſto *."

The exiſtence of this fluid in the bamboo is known by ſhaking the joint. In a conſiderable number of bamboos ſplit in order to procure it, I never found water in more than two joints, and generally not more than two or three drams in each; the largeſt quantity procured at one time was one ounce and a half. Very few joints in proportion contained any.

The fluid was always tranſparent, but varied in conſiſtence; when thicker it had a whiter colour than common; when more dilute it differed little to the eye from common water, or ſometimes had a pale greeniſh caſt. Applied to the tongue and palate, it had a ſlight ſaline, ſub-aſtringent taſte, more or leſs perceptible in proportion to the conſiſtence of the fluid. After evaporation in the ſun, the reſiduum had a pretty ſtrong ſaline taſte, with leſs aſtringency. Some of the fluid, of a darkiſh colour, thickened in the reed to the conſiſtence of honey; and ſome, in another joint of the ſame reed, was perfectly white and almoſt dry: both had the ſharp ſalt taſte, which the Tabaſheer itſelf loſes in a great degree by keeping.

From two green bamboos, each of five joints, which had been cut only a few days before, I procured above two ounces

* Capitolo XII.

of

of fluid; it had a ſlight ſaline taſte, and in colour had a greeniſh caſt.

One ounce was put into a phial, N° 1. and about ten drams into another phial, N° 2.; both were ſtopped with glaſs ſtoppers. After two days they had both depoſited a ſmall ſediment; but the ſediment in N° 1. was three times more than that in the other. At the end of the week, the water in both was found ſweet, and the ſediment increaſed, but moſt in N° 1.

At the end of a fortnight, the water in N° 1. had a fetid ſmell, with a whitiſh cottony ſediment, and a thin film of the ſame kind ſuſpended at top. The whole, well ſhaken together, was poured into a glaſs veſſel, and left to evaporate ſlowly. The reſiduum conſiſted of ſmall particles of a whitiſh brown colour, reſembling the inferior ſort of Tabaſheer.

The water in N° 2. had hardly any fetid ſmell at this time; and at the end of the month remained in the ſame ſtate: the ſediment had increaſed very little.

The recent green bamboos, which, upon ſhaking, appeared to contain water in the cavity, loſt this appearance after ſtanding a few days, ſome ſooner, ſome later. When ſplit, after they no longer gave any ſound by ſhaking, ſometimes no fluid was found in the cavity, as if the whole had eſcaped. The interior thin pellicle, however, was diſcoloured, as if by recent moiſture; but generally ſome of the fluid remained in a mucilaginous ſtate, more or leſs thick, at the lower part of the joint. It may be remarked, that ſmall worms were ſometimes found in the ſame joints with the water, which ſurvived ſeveral hours, ſwimming about in the water after its extraction.

In the latter end of October, a green bamboo of five joints was brought to me, which appeared to contain both water and

Tabaſheer. After three days, the ſound of water, upon ſhaking the reed, could hardly be perceived; on the fifth day it was intirely imperceptible.

Upon ſplitting the bamboo, about half a dram of the fluid, now thickened into a mucilage, was found at the bottom of the upper joint. The ſecond joint contained ſome perfect Tabaſheer looſe in the cavity. The third joint was empty, excepting a few particles of Tabaſheer, which adhered to the ſides near the bottom. The fourth joint, at the bottom, contained above a dram of a browniſh pulpy ſubſtance, adherent. The laſt joint, in like manner, contained half a dram of a ſubſtance thicker and harder in conſiſtence, and nearly of the colour of white wax.

This ſpecimen exhibited at one view the progreſs of the Tabaſheer through its ſeveral ſtages. The ſound diſtinctly perceived in the firſt joint on the 23d of October was produced by the water in a fluid ſtate; on the 31ſt, having become thicker, the ſound, upon ſhaking, was very obſcure; on the 2d of November, no ſound was perceptible; and when the reed was ſplit, the water was found reduced to a mucilage. The fourth and fifth joints contained the drug in a more advanced ſtate. In the firſt, it was thicker than a mucilage of a browniſh colour; in the ſecond, more of the fluid part having evaporated, the colour was whiter, and it wanted but little of the conſiſtence of the perfect Tabaſheer found in the ſecond joint.

I am, &c.

PAT. RUSSELL.

Vizagapatam,
Nov. 26, 1788.

POST-

POSTSCRIPT.

Weymouth Street, July 16, 1790.

FOUR of the ſeven reeds preſented to the Society on the night this Paper was read, being carefully ſplit, the contents, upon comparing them with the ſpecimens ſent from India, then on the table, were found to agree in all reſpects, as well as with the deſcription of the more recent drug given in the above Paper. The ſpecimen, N° 3. ſent from Hydrabad, and reckoned the prime ſort, differed ſomewhat in hardneſs, as mentioned above, from the pureſt particles in the Tabaſheer collected by myſelf; but in the opinion of ſeveral of the Members preſent, who compared them, were the ſame ſubſtance with the particles mixed, in a ſmall proportion, in ſome of the other ſpecimens, as likewiſe with a few particles taken from the reeds opened in their preſence; which puts it beyond doubt, that the ſubſtance is produced in the cavity of the Bamboo.

The ſeveral ſpecimens are now under chemical trial; and the reſult of the experiments will, I hope, be communicated to the Society.

XIV. *Account of the Nardus Indica, or Spikenard.*

By Gilbert Blane, *M. D. F. R. S.*

Read March 18, 1790.

IT is much to be regretted, that the records of antiquity afford ſuch imperfect deſcriptions of natural objects, particularly of thoſe of the vegetable kingdom. Moſt of the writings of the ancients have come down to us either mutilated by the accidents of time, or diſtorted and corrupted by unfaithful and ignorant tranſcribers. There is reaſon to think, that the learned works upon profeſſional ſubjects have been more unfortunate in theſe reſpects than works of imagination and general ſcience, for the former are in fact more obſcure and confuſed; and as they would be leſs generally intereſting, and leſs intelligible, to tranſcribers, they would of courſe be more liable to neglect and miſtake. But ſuppoſing the works of THEOPHRASTUS, DIOSCORIDES, and the other ancient phyſicians and naturaliſts, to be extant in their utmoſt completeneſs and purity; their method of deſcribing plants and other natural bodies was ſo defective, that very few of them could now be recognized. We have not only to contend with the obſcurity belonging to a dead language, in ſo far as the name merely is concerned; but it would be impoſſible, even in a living language, to perpetuate the knowledge of any object in nature, ſuch as a plant, without ſome deſcription to diſcriminate it

from

from all others. For want of ſuch deſcriptions, the knowledge contained in the writings of the ancient naturaliſts could be of uſe only to their contemporaries and countrymen, who were already acquainted with the objects of it, and could afford no certain information to the ignorant in diſtant countries and future ages. Of all the ancient medicines, there is perhaps none but opium, of which the identity can be unqueſtionably aſcertained. Of moſt of them little more is ſaid than merely giving their names. But while nature is conſtant and invariable, language is local and fluctuating, ſo that ſome of the moſt important knowledge, though committed to writing, reſts upon little better foundation than if it were vague tradition. In conſequence of this ambiguity, the fruits of the ingenuity and labour of one age have, in a great meaſure, been loſt to another; and uſeful inventions in various arts have become extinct, for want of the means of aſcertaining the materials employed in them. Poſterity will therefore be greatly indebted to thoſe induſtrious naturaliſts of the preſent times who are carrying the deſcription of nature to an unexampled height of improvement; and our lateſt deſcendants will be enabled, without difficulty, to avail themſelves of the accumulated experience of preceding ages.

I have been led to theſe reflexions by an account, ſent me ſome time ago by my brother in India, of the Spikenard, or *Nardus Indica*, a name familiar in the works of the ancient phyſicians, naturaliſts, and poets; but the identity of which has not hitherto been ſatisfactorily aſcertained. He ſays, in a letter dated Lucknow, December 1786, that, "travelling "with the Nabob Viſier, upon one of his hunting excurſions "towards the northern mountains, I was ſurpriſed one day, "after croſſing the river Rapty, about twenty miles from the

"foot.

" foot of the hills, to perceive the air perfumed with an aro-
" matic ſmell; and, upon aſking the cauſe, I was told it pro-
" ceeded from the roots of the graſs that were bruiſed or
" trodden out of the ground by the feet of the elephants and
" horſes of the Nabob's retinue. The country was wild and
" uncultivated, and this was the common graſs which covered
" the ſurface of it, growing in large tufts cloſe to each other,
" very rank, and in general from three to four feet in length.
" As it was the winter ſeaſon, there was none of it in flower.
" Indeed, the greateſt part of it had been burnt down on the
" road we went, in order that it might be no impediment to
" the Nabob's encampments.

" I collected a quantity of the roots to be dried for uſe, and
" carefully dug up ſome of it, which I ſent to be planted in my
" garden at Lucknow. It there throve exceedingly, and in
" the rainy ſeaſon it ſhot up ſpikes about ſix feet high. Ac-
" companying this, I ſend you a drawing of the plant in flower,
" and of the dried roots, in which the natural appearance is
" tolerably preſerved.

" It is called by the natives *Terankus*, which means literally,
" in the Hindoo language, fever-reſtrainer, from the virtues
" they attribute to it in that diſeaſe. They infuſe about a
" dram of it in half a pint of hot water, with a ſmall quan-
" tity of black pepper. This infuſion ſerves for one doſe, and
" is repeated three times a day. It is eſteemed a powerful
" medicine in all kinds of fevers, whether continued or inter-
" mittent. I have not made any trial of it myſelf; but ſhall
" certainly take the firſt opportunity of doing ſo.

" The whole plant has a ſtrong aromatic odour; but both
" the ſmell and the virtues reſide principally in the huſky
" roots, which in chewing have a bitter, warm, pungent taſte,

"accompanied with ſome degree of that kind of glow in the "mouth which cardamoms occaſion."

Beſides the drawing, a dried ſpecimen has been ſent, which was in ſuch good preſervation as to enable Sir JOSEPH BANKS, P. R. S. to aſcertain it by the botanical characters to be a ſpecies of *Andropogon*, different from any plant that has uſually been imported under the name of Nardus, and different from any of that genus hitherto deſcribed in botanical ſyſtems.

There is great reaſon, however, to think, that it is the true *Nardus Indica* of the ancients; for, firſt, the circumſtance, in the account above recited, of its being diſcovered in an unfrequented country from the odour it exhaled by being trod upon by the elephants and horſes, correſponds, in a ſtriking manner, with an occurrence related by ARRIAN, in his Hiſtory of the Expedition of Alexander the Great into India. It is there mentioned, lib. VI. cap. 22. that, during his march through the deſarts of Gadroſia, the air was perfumed by the Spikenard, which was trampled under foot by the army; and that the Phœnicians, who accompanied the expedition, collected large quantities of it, as well as of myrrh, in order to carry them to their own country, as articles of merchandiſe. This laſt circumſtance ſeems further to aſcertain it to have been the true Nardus; for the Phœnicians, who, even in war, appear to have retained their genius for commerce, could no doubt diſtinguiſh the proper quality of this commodity. I am informed by Major RENNELL, F. R. S. whoſe accurate reſearches in Indian geography are ſo well known to the publick, that Gadroſia or Gedroſia anſwers to the modern Mackran or Kedge-Mackran, a maritime province of Perſia, ſituated between Kerman (the ancient Carmania) and the river Indus, being of courſe the frontier of Perſia towards India; and that

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it appears from ARRIAN's account, and from a Turkiſh map of Perſia, that this deſart lies in the middle of the tract of country between the river Indus and the Perſian Gulph, and within a few days march of the Arabian or Erythræan ſea *. It would appear, that the Nard was found towards the eaſtern part of it; for ALEXANDER was then directing his route to the weſtward, and the length of march through the deſart afterwards was very great, as they were obliged to kill their beaſts of burden in conſequence of their ſubſequent diſtreſs. Secondly, though the accounts of the ancients concerning this plant are obſcure and defective, it is evident, it was a plant of the order of *gramina*; for the term *ariſta*, ſo often applied to it, was appropriated by them to the fructification of grains and graſſes, and ſeems to be a word of Greek original to denote the moſt excellent portion of theſe plants, which are the moſt uſeful in the vegetable creation for the ſuſtenance of animal life, and nature has alſo kindly made them the moſt abundant in all parts of the habitable earth. The term *ſpica* is applied to plants of the natural order *verticillatæ*, in which there are many ſpecies of fragrant plants, and the lavender, which being an indigenous one, affording a grateful perfume, was called *Nardus Italica* by the Romans; but we never find the term *ariſta* applied to theſe. The poets, as well as the naturaliſts, conſtantly apply this latter term to the true Nardus. STATIUS calls the Spikenard *odoratæ ariſtæ*. OVID, in mentioning it as one of the materials of the Phœnix's neſt, calls it *Nardi levis ariſta*; and a poem, aſcribed to LACTANTIUS, on the

* By the Erythræan Sea the ancients meant the northern part of the Æthiopic Ocean, waſhing the ſouthern coaſts of Arabia and Perſia, and not, as the name would imply, what is, in modern times, called the Red Sea. The ancient name of the Red Sea was Sinus Arabicus.

ſame

ſame ſubject, ſays, *his addit teneras Nardi pubentis ariſtas*, where the epithet *pubentis* ſeems even to point out that it belonged to the genus *andropogon*, a name given to it by LINNÆUS from this circumſtance. GALEN ſays, that though there are various ſorts of *Nardus*, the term Ναρδο-σαχυς, or Spikenard, ſhould not be applied to any but the *Nardus Indica*. It would appear, that the *Nardus Celtica* was a plant of a quite different habit, and is ſuppoſed to be a ſpecies of *Valeriana*. The deſcription of the *Nardus Indica* by PLINY does not indeed correſpond with the appearance of our ſpecimen; for he ſays it is *frutex radice pingui et craſſâ*; whereas ours has ſmall fibrous roots. But as Italy is very remote from the native country of this plant, it is reaſonable to ſuppoſe, that others, more eaſily procurable, uſed to be ſubſtituted for it; and the ſame author ſays, that there were nine different plants by which it could be imitated and adulterated. There would be ſtrong temptations to do this from the great demand for it, and the expence and difficulty of diſtant inland carriage; and as it was much uſed as a perfume, being brought into Greece and Italy in the form of an unguent manufactured in Laodicea, Tarſus, and other towns of Syria and Aſia Minor, it is probable, that any grateful aromatic reſembling it was allowed to paſs for it. It is probable, that the Nardus of PLINY, and great part of what is now imported from the Levant, and found under that name in the ſhops, is a plant growing in the countries on the Euphrates, or in Syria, where the great emporiums of the eaſtern and weſtern commerce were ſituated. There is a *Nardus Aſſyria* mentioned by HORACE; and DIOSCORIDES mentions the *Nardus Syriaca*, as a ſpecies different from the *Indica*, which certainly was brought from ſome of the remote parts of India; for both DIOSCORIDES and GALEN,

by way of fixing more preciſely the country from whence it comes, call it alſo *Nardus Gangites*. Thirdly, GARCIAS AB HORTO, a Portugueſe, who reſided many years at Goa in the ſixteenth century, has given a figure of the roots, or rather the lower parts of the ſtalks, which correſponds with our ſpecimen; and he ſays expreſſly, that there is but this one ſpecies of *Nardus* known in India, either for the conſumption of the natives, or for exportation to Perſia and Arabia. It is remarkable, that he is perhaps the only author who ſpeaks of it in its recent ſtate from his own obſervation. It is not to be met with among the many hundreds of plants delineated in the *Hortus Malabaricus*. The Schœnanthus of RUMPHIUS does not correſpond with it, being only one palm in height; but he mentions having ſeen a dried ſpecimen of it, of which the leaves were almoſt five feet high; and that Mackran was one of the countries from whence it was brought. This muſt be the ſame as that mentioned by ARRIAN, but differs from that of GARCIAS in the length of the ſtalks; but this might be either becauſe the meaſure was taken at different ſeaſons of the year, for the ſpecimen before us was much ſhorter in winter than when it ſhot into ſpikes, or becauſe that of GARCIAS being, according to his own account, cultivated, it might not be ſo luxuriant as that which grew ſpontaneous in its native ſoil. Fourthly, the ſenſible qualities of this are ſuperior to what commonly paſſes for it in the ſhops, being poſſeſſed both of more fragrancy and pungency, which ſeems to account for the preference given to it by the ancients.

There is a queſtion concerning which MATHIOLUS, the commentator of DIOSCORIDES, beſtows a good deal of argument, *viz.* whether the roots or ſtalks were the parts eſteemed for uſe, the teſtimony of the ancients themſelves on this point

being ambiguous. The roots of this ſpecimen are very ſmall, and poſſeſs ſenſible qualities inferior to the reſt of the plant; yet it is mentioned in the account above recited, that the virtues reſide principally in the *huſky roots*. It is evident, that by the huſky roots muſt here be meant the lower parts of the ſtalks and leaves where they unite to the roots; and it is probably a ſlight inaccuracy of this kind that has given occaſion to the ambiguity that occurs in the ancient accounts.

With regard to the virtues of this plant, it was highly valued anciently as an article of luxury as well as a medicine. The favourite perfume which was uſed at the ancient baths and feaſts was the *unguentum nardinum*; and it appears, from a paſſage in HORACE, that it was ſo valuable, that as much of it as could be contained in a ſmall box of precious ſtone was conſidered as a ſort of equivalent for a large veſſel of wine, and a handſome quota for a gueſt to contribute at an entertainment, according to the cuſtom of antiquity:

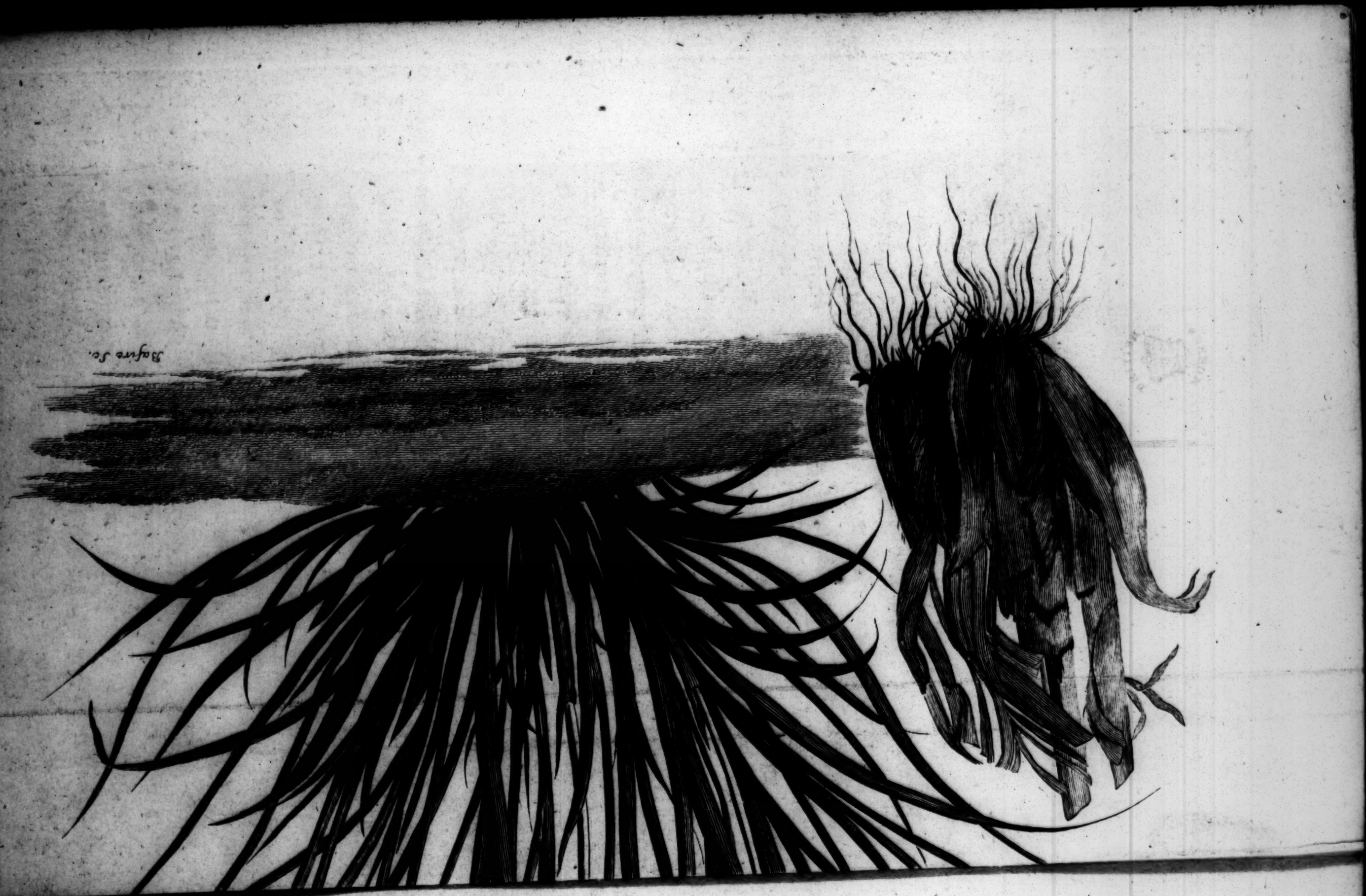
——— *Nardo vinum merebere*
Nardi parvus onyx eliciet cadum.

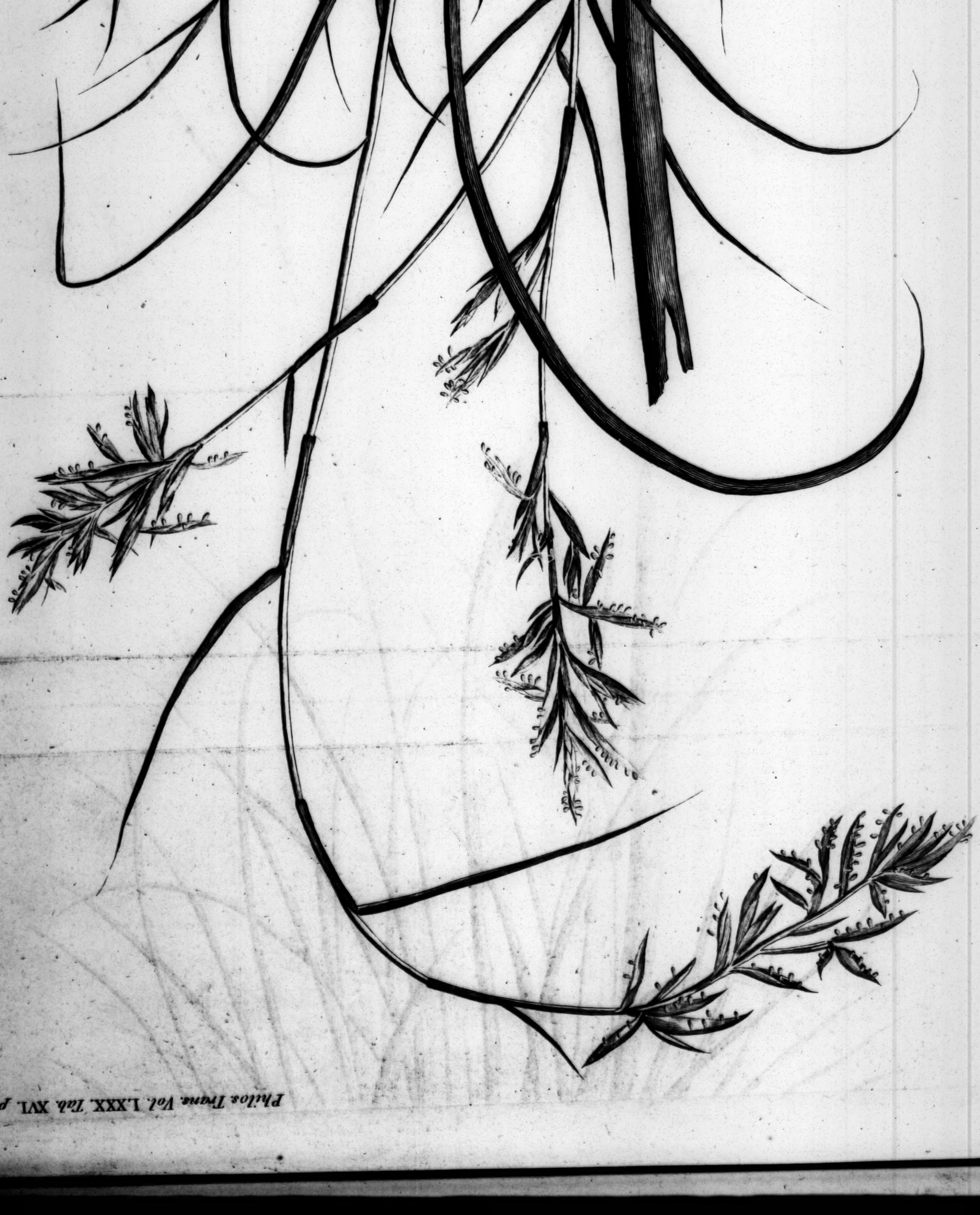
It may here be remarked, that as its ſenſible qualities do not depend on a principle ſo volatile as eſſential oil, like moſt other aromatic vegetables, this would be a great recommendation to the ancients, as its virtues would thereby be more durable, and they were not acquainted with the method of collecting eſſential oils, being ignorant of the art of diſtillation. The fragrance and aromatic warmth of the *Nardus* depends on a fixed principle like that of cardamoms, ginger, and ſome other ſpices. I tried to extract the virtues of the *Nardus* by boiling water, by maceration in wine and in proof ſpirits, but it yielded them but ſparingly and with difficulty to all theſe *menſtrua*.

It had a high character among the ancients as a remedy both external and internal. It is one in the list of ingredients in all the antidotes, from those of Hippocrates, as given on the authority of Myrepsus and Nicolaus Alexandrinus, to the officinals which have kept their ground till modern times under the names of Mithridate and Venice Treacle. It is recommended by Galen and Alexander Trallian in the dropsy and gravel. Celsus and Galen recommend it both externally and internally in pains of the stomach and bowels. The first occasion on which the latter was called to attend Marcus Aurelius was when that Emperor was severely afflicted with an acute complaint in the bowels, answering by the description to what we now call *cholera morbus*; and the first remedy he applied was warm *Oleum nardinum* on wool to the stomach. He was so successful in the treatment of this illness, that he ever afterwards enjoyed the highest favour and confidence of the Emperor.

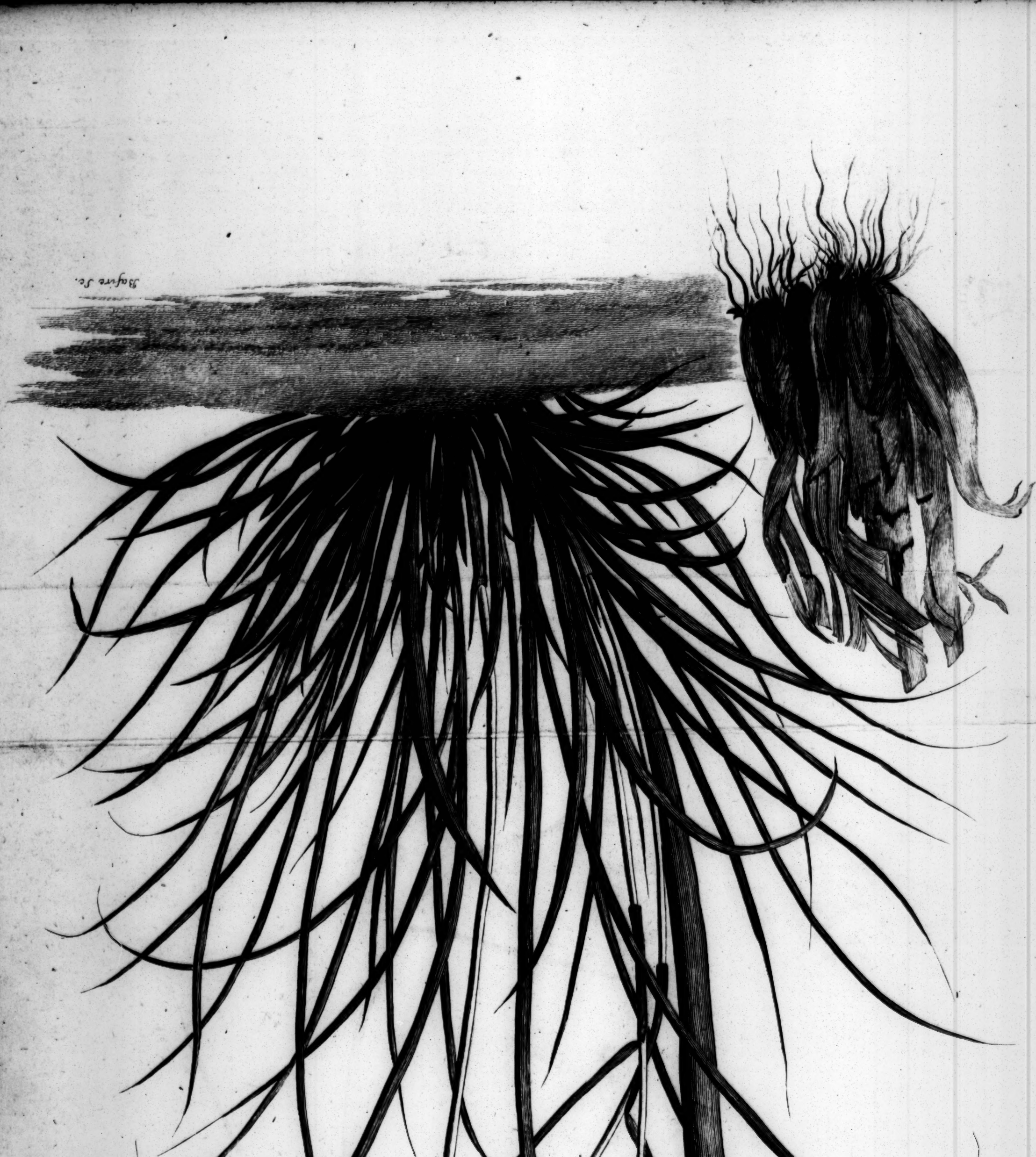
It would appear, that the natives of India consider it as an efficacious remedy in fevers, and its sensible qualities promise virtues similar to those of other simples now in use among us in such cases. Besides a strong aromatic flavour, it possesses a pungency to the taste little inferior to the *serpentaria*, and much more considerable than the *contrayerva*. It is mentioned in a work attributed to Galen, that a medicine, composed of this and some other aromatics, was found useful in long protracted fevers, which are the cases in which medicines of this class are employed in modern practice.

Tab. XVI. is a representation of the Plant.





Philos. Trans. Vol. LXXX. Tab. XVI. p. 192.



XV. *An Account of ſome extraordinary Effects of Lightning.*

By William Withering, *M. D. F. R. S.*

Read March 18, 1790.

PERMIT me to requeſt the attention of the Royal Society, whilſt I mention a few facts relative to a thunder cloud, the lightning from which fuſed a quantity of quartzoſe matter.

This cloud formed in the ſouth, in the afternoon of Sept. 3, 1789, and took its courſe nearly due north. In its paſſage it ſet fire to a field of ſtanding corn; but the rain preſently extinguiſhed the fire. Soon afterwards the lightning ſtruck an oak tree, in the Earl of AYLESFORD's park at Packington.

The heighth of this tree is 39 feet, including its trunk, which is 13 feet. It did not ſtrike the higheſt bough, but that which projected fartheſt ſouthward. A man, who had taken ſhelter againſt the north ſide of the tree, was ſtruck dead inſtantaneouſly, his cloaths ſet on fire, and the moſs *(lichen)* on the trunk of the tree, where the back of his head had reſted, was likewiſe burnt. Two men, ſpectators of the accident, ran immediately towards him upon ſeeing him fall; and as it rained hard, and a ſmall lake had collected almoſt cloſe to the ſpot, the fire was very ſoon extinguiſhed; but the effects of the fire on one-half of his body, and on his cloaths, were ſuch as to ſhew, that the whole burning was inſtantaneous, not progreſſive.

Part of the electric matter paſſed down a walking ſtick, which the man held in his hand, ſloping from him; and where the ſtick reſted on the ground, it made a perforation about 2½ inches in diameter, and 5 inches deep. This hole I examined ſoon afterwards, and found nothing in it but the burnt roots of the graſs. All obſervation would probably have ended here, had not Lord AYLESFORD determined to erect a monument upon the ſpot, not merely to commemorate the event, but with an inſcription, to caution the unwary againſt the danger of ſheltering under a tree during a thunder ſtorm. In digging the foundation for this monument, the earth was diſturbed at the perforation before mentioned, and the ſoil appeared to be blackened to the depth of about ten inches. At this depth, a root of the tree preſented itſelf, which was quite black; but this blackneſs was only ſuperficial, and did not extend far along it. About two inches deeper, the melted quartzoſe matter began to appear, and continued in a ſloping direction to the depth of 18 inches.

The ſpecimens which accompany this Paper, and for which I am indebted to the attention of Lord AYLESFORD, will demonſtrate the intenſe heat which muſt have exiſted to bring ſuch materials into fuſion.

N° 1. A quartz pebble, one corner of which has been completely fuſed.

N° 2. Sand, unmixed with calcareous matter, agglutinated by the heat. Within the hollow part of this maſs, the fuſion has been ſo perfect, that the melted quartzoſe matter has run down the hollow, and aſſumed nearly a globular figure.

N° 3. Smaller hollow pieces, and one nearly flat, but all the flat ones have ſome hollow part *.

* Theſe ſpecimens were laid before the Society when the Paper was read. C. B.

Mr.

Mr. WATT ſuggeſted to me, that the hollows had been occaſioned by the expanſion of moiſture whilſt the fuſion exiſted.

I ſhall conclude with obſerving, that judging from the damage done to the oak tree, the ſtroke was not very great; and that having now an inducement to dig where the earth has been perforated by lightning, we may probably hereafter find foſſil ſubſtances melted by it to a conſiderably greater extent.



XVI. *An Account of a Child with a double Head. In a Letter from* Everard Home, *Esq. F. R. S. to* John Hunter, *Esq. F. R. S.*

Read March 25, 1790.

DEAR SIR,

I FEEL a particular ſatisfaction in having been enabled, through the kind attention of my Friend Captain Buchanan, to add to your invaluable collection the very uncommon double ſkull of a monſtrous child, born in the Eaſt Indies, which attracted the attention of all the curious in Calcutta, where it was ſhewn alive; and, ſhould the following account of it appear to you of ſufficient importance, I ſhall requeſt that you will do me the honour of laying it before the Royal Society.

It is much to be regretted, that the hiſtories of monſtrous appearances in the ſtructure of the human body which are to be found in the works of the older writers, and even of many of the moderns, are ſo little to be depended upon. Few authors have contented themſelves with giving a ſimple detail of facts that were extraordinary; but, from an over anxiety to make them ſtill more wonderful, or from having given an implicit belief to the accounts received from the credulous and ignorant, they have commonly added circumſtances too extravagant to deſerve the attention of a reaſonable mind, which

prevent

prevent the reader from giving credit to any part of the narration. This has been ſo general, that whenever the hiſtory of any thing uncommon appears, the mind is impreſſed with a doubt of its authenticity, and requires ſome ſtronger evidence of the facts than the ſingle teſtimony of an individual in other reſpects unimpeached in his veracity.

As the hiſtories of remarkable deviations from the common courſe of nature in the formation of the human body already regiſtered in the Philoſophical Tranſactions are very numerous, I am deſirous of adding to them an account of one ſo truly uncommon, that, I believe, no ſimilar inſtance is to be found upon record. It is a ſpecies of *luſus naturæ* ſo unaccountable, that, although the facts are ſufficiently eſtabliſhed by the teſtimonies of the moſt reſpectable witneſſes, I ſhould ſtill be diffident in bringing them before the Royal Society, were I not enabled at the ſame time to produce the double ſkull itſelf, in which the appearances illuſtrate ſo clearly the different parts of the hiſtory that it muſt be rendered perfectly ſatisfactory to the minds of the moſt incredulous.

The following account of the child, when ſix months old, I was favoured with from Sir JOSEPH BANKS; who, from the hand-writing, and other circumſtances, believes that it was written by the late Colonel PIERCE. I have, however, been leſs ſolicitous to aſcertain the author, as the obſervations contained in this account agree ſo intirely with the remarks that were afterwards made, and with the appearances of the ſkull, that they require no name being annexed to them, in confirmation of their having been made with accuracy and fidelity.

The child was born in May, 1783, of poor parents; the mother was thirty years old, and named NOOKI; the father

was called HANNAI, a farmer at Mandalgent near Bardawan, in Bengal, and aged thirty-five.

At the time of the child's birth, the woman who acted as midwife, terrified at the ſtrange appearance of the double head, endeavoured to deſtroy the infant by throwing it upon the fire, where it lay a ſufficient time before it was removed to have one of the eyes and ears conſiderably burnt.

The body of the child was naturally formed, but the head appeared double, there being, beſides the proper head of the child, another of the ſame ſize, and to appearance almoſt equally perfect, attached to its upper part. This upper head was inverted, ſo that they ſeemed to be two ſeparate heads united together by a firm adheſion between their crowns, but without any indentation at their union, there being a ſmooth continued ſurface from the one to the other. The face of the upper head was not over that of the lower, but had an oblique poſition, the center of it being immediately above the right eye.

When the child was ſix months old, both of the heads were covered with black hair, in nearly the ſame quantity. At this period the ſkulls ſeemed to have been completely oſſified, except a ſmall ſpace between the oſſa frontis of the upper one, like a fontinelle.

Obſervations on the ſuperior or inverted head.

No pulſation could be felt in the ſituation of the temporal arteries; but the ſuperficial veins were very evident.

The neck was about two inches long, and the upper part of it terminated in a rounded ſoft tumor, like a ſmall peach.

One of the eyes had been conſiderably hurt by the fire, but the other appeared perfect, having its full quantity of motion; but the eyelids were not thrown into action by any thing ſuddenly approaching the eye; nor was the iris at thoſe times in

the leaſt affected; but, when ſuddenly expoſed to a ſtrong light, it contracted, although not ſo much as it uſually does. The eyes did not correſpond in their motions with thoſe of the lower head; but appeared often to be open when the child was aſleep, and ſhut when it was awake.

The external ears were very imperfect, being only looſe folds of ſkin; and one of them mutilated by having been burnt. There did not appear to be any paſſage leading into the bone which contains the organ of hearing.

The lower jaw was rather ſmaller than it naturally ſhould be, but was capable of motion. The tongue was ſmall, flat, and adhered firmly to the lower jaw, except for about half an inch at the tip, which was looſe. The gums in both jaws had the natural appearance; but no teeth were to be ſeen either in this head or the other.

The internal ſurfaces of the noſe and mouth were lubricated by the natural ſecretions, a conſiderable quantity of mucus and ſaliva being occaſionally diſcharged from them.

The muſcles of the face were evidently poſſeſſed of powers of action, and the whole head had a good deal of ſenſibility, ſince violence to the ſkin produced the diſtortion expreſſive of crying, and thruſting the finger into the mouth made it ſhew ſtrong marks of pain. When the mother's nipple was applied to the mouth, the lips attempted to ſuck.

The natural head had nothing uncommon in its appearance; the eyes were attentive to objects, and its mouth ſucked the breaſt vigorouſly. Its body was emaciated.

The parents of the child were poor, and carried it about the ſtreets of Calcutta as a curioſity to be ſeen for money; and to prevent its being expoſed to the populace, they kept it conſtantly covered up, which was conſidered as the cauſe of its being emaciated and unhealthy.

The attention of the curious was naturally attracted by ſo uncommon a ſpecies of deformity; and Mr. STARK, who reſided in Bengal during this period, paid particular attention to the appearances of the different parts of the double head, and endeavoured to aſcertain the mode in which the two ſkulls were united, as well as to diſcover the ſympathies which exiſted between the two brains. Upon his return to England, finding that I was in poſſeſſion of the ſkull, and propoſed drawing up an account of the child, he very obligingly favoured me with the following particulars, and has likewiſe allowed me to have a ſketch taken from a very exact painting, made under his own inſpection from the child while alive, by Mr. SMITH, a portrait painter then in India. From this drawing, which is annexed, and two others, repreſenting the heads in the natural ſtate; and the ſkulls, when all the other parts were removed, a much more accurate idea will be given of the child's appearance than can be conveyed by any deſcription.

At the time Mr. STARK ſaw the child, it muſt have been nearly two years old *, as it was ſome months before its death, which I have every reaſon to believe happened in the year 1785. At this period the appearances differed in many reſpects from thoſe taken notice of when only ſix months old.

The burnt ear had ſo much recovered itſelf as only to have loſt about one fourth part of the looſe pendulous flap. The openings leading from the external ear appeared as diſtinct as in thoſe of the other head. The ſkin ſurrounding the injured eye, which was on the ſame ſide with the mutilated ear, was in a

* The dentes molares, or double teeth, which uſually appear at twenty months or two years of age, were through the gum; and there was no reaſon to expect them very early in this child.

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ſlight degree affected, and the external canthus much contracted, but the eye itſelf was perfect.

The eyelids of the ſuperior head were never completely ſhut, remaining a little open, even when the child was aſleep, and the eyeballs moved at random. When the child was rouſed, the eyes of both heads moved at the ſame time; but thoſe of the ſuperior head did not appear to be directed to the ſame object, but wandered in different directions. The tears flowed from the eyes of the ſuperior head almoſt conſtantly, but never from the eyes of the other, except when crying.

The termination of the upper neck was very irregular, a good deal reſembling the cicatrix of an old ſore.

The ſuperior head ſeemed to ſympathiſe with the child in moſt of its natural actions. When the child cried, the features of this head were affected in a ſimilar manner, and the tears flowed plentifully. When it ſucked the mother, ſatisfaction was expreſſed by the mouth of the ſuperior head, and the ſaliva flowed more copiouſly than at any other time; for it always flowed a little from it. When the child ſmiled, the features of the ſuperior head ſympathiſed in that action. When the ſkin of the ſuperior head was pinched, the child ſeemed to feel little or no pain, at leaſt not in the ſame proportion as was felt from a ſimilar violence being committed on its own head or body.

When the child was about two years old, and in perfect health, the mother went out to fetch ſome water; and, upon her return, found it dead, from the bite of a *Cobra de capelo*. The parents at this time lived upon the grounds of Mr. DENT, the honourable Eaſt India Company's Agent for Salt at Tumloch, and the body was buried near the banks of the Boopnorain river. It was afterwards dug up by Mr. DENT

and his European ſervant, the religious prejudices of the parents not allowing them to diſpenſe with its being interred.

My friend, Captain BUCHANAN, late Commander of the Ranger Packet, in the ſervice of the honourable the Eaſt India Company, when at Bengal reſided a few days in Mr. DENT's houſe. He was much ſtruck with the uncommon appearance of the double ſkull, and expreſſed a wiſh that he might be allowed to bring it to Europe, and preſent it to me; knowing, from the intereſt I have always taken in thoſe purſuits which have ſo long and ſo deeply engaged your attention, it would be a moſt acceptable preſent. His requeſt was no ſooner communicated to Mr. DENT, than it was complied with; that gentleman having too much liberality to heſitate a moment in ſending ſo rare a curioſity to Europe. I ſhould do both theſe gentlemen injuſtice, were I not to attribute their readineſs upon the preſent occaſion to oblige me, in a great meaſure to their knowing that the double ſkull would be depoſited in your collection, which muſt now be conſidered more as a national and public repoſitory than a private cabinet.

The two ſkulls which compoſe this monſtrous head appear to be nearly of the ſame ſize, and equally complete in their oſſification, except a ſmall ſpace at the upper edge of the oſſa frontis of the ſuperior ſkull, ſimilar to a fontinelle. The mode in which the two are united is curious, as no portion of bone is either added or diminiſhed for that purpoſe; but the frontal and parietal bones of each ſkull, inſtead of being bent inwards, ſo as to form the top of the head, are continued on; and, from the oblique poſition of the two heads, the bones of the one paſs a little way into the natural ſutures of the other, forming a zig-zag line, or circular ſuture uniting them together.

The

The two ſkulls appear to be almoſt equally perfect at their union; but the ſuperior ſkull, as it recedes from the other, is becoming more imperfect and deficient in many of its parts.

The meatus auditorius in the temporal bone is altogether wanting.

The baſis of the ſkull is imperfect in ſeveral reſpects, particularly in ſuch parts as are to connect the ſkull with a body. The foramen magnum occipitale is a ſmall irregular hole, very inſufficient to give paſſage to a medulla ſpinalis; round its margin are no condyles with articulating ſurfaces, as there were no vertebræ of the neck to be attached to it. The foramen lacerum in baſi cranii is only to be ſeen on one ſide, and even there too ſmall for the jugular vein to have paſſed through.

The oſſa palati are deficient at their poſterior part; the lower jaw is too ſmall for the upper, and the condyle and coronoid proceſs of one ſide are wholly wanting.

In moſt of the other reſpects, the two ſkulls are alike; the number of teeth in both is the ſame, and is ſixteen.

From an examination of the internal ſtructure of the double ſkull, the two brains have certainly been incloſed in one bony caſe, there being no ſeptum of bone between them. How far they were intirely diſtinct, and ſurrounded by their proper membranes, cannot now be aſcertained; but from the ſympathies which were taken notice of by Mr. STARK between the two heads, more particularly thoſe of the ſuperior with the lower, or more perfect, I ſhould be inclined to believe, that there was a more intimate connection between them than ſimply by means of nerves, and therefore that the ſubſtance of the brains was continued into one another.

Had the child lived to a more advanced age, and given men of obſervation opportunities of attending to the effects of this

double

double brain, its influence upon the intellectual principle muſt have afforded a curious and uſeful ſource of inquiry; but unfortunately the child only lived long enough to complete the oſſification of the ſkull ſo as to retain its ſhape, by which means we have been enabled to aſcertain and regiſter the fact, without having enjoyed the ſatisfaction that would have reſulted from an examination of the brain itſelf, and a more mature inveſtigation of the effects it would have produced.

Yours, &c.

E. HOME.

Leiceſter-ſquare,
May 22, 1790.

EXPLANATION OF THE PLATES.

Tab. XVII. The child is repreſented in this plate as it appeared at the age of twenty months, and is copied from a picture in the poſſeſſion of Mr. STARK.

The painting was taken from the child ſix months before its death by Mr. SMITH, an ingenious artiſt, at that time reſiding in Bengal.

It conveys a general idea of the appearance of this extraordinary child, and the relative proportions between the double head and the body.

Tab. XVIII. fig. 1. In this figure the double head is repreſented exactly half the natural ſize. One of the eyes of the upper face appears ſmaller or more contracted than the other; this is in conſequence of the injury it received when the child was thrown upon the fire.

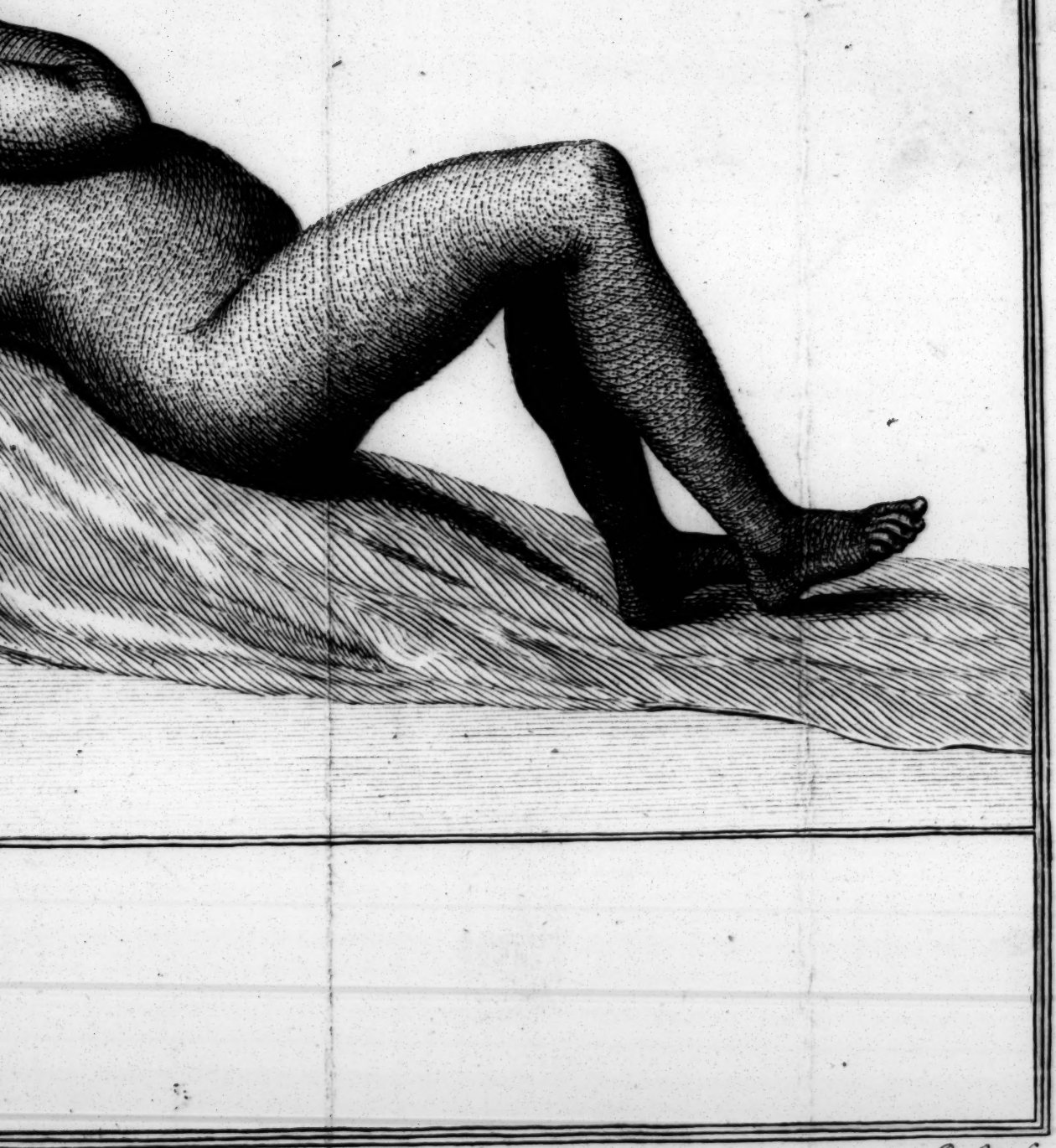
The



W.m Bell del.



Wm Bell del.



Basire. Sc.



Fig. I.

Wm Bell del.

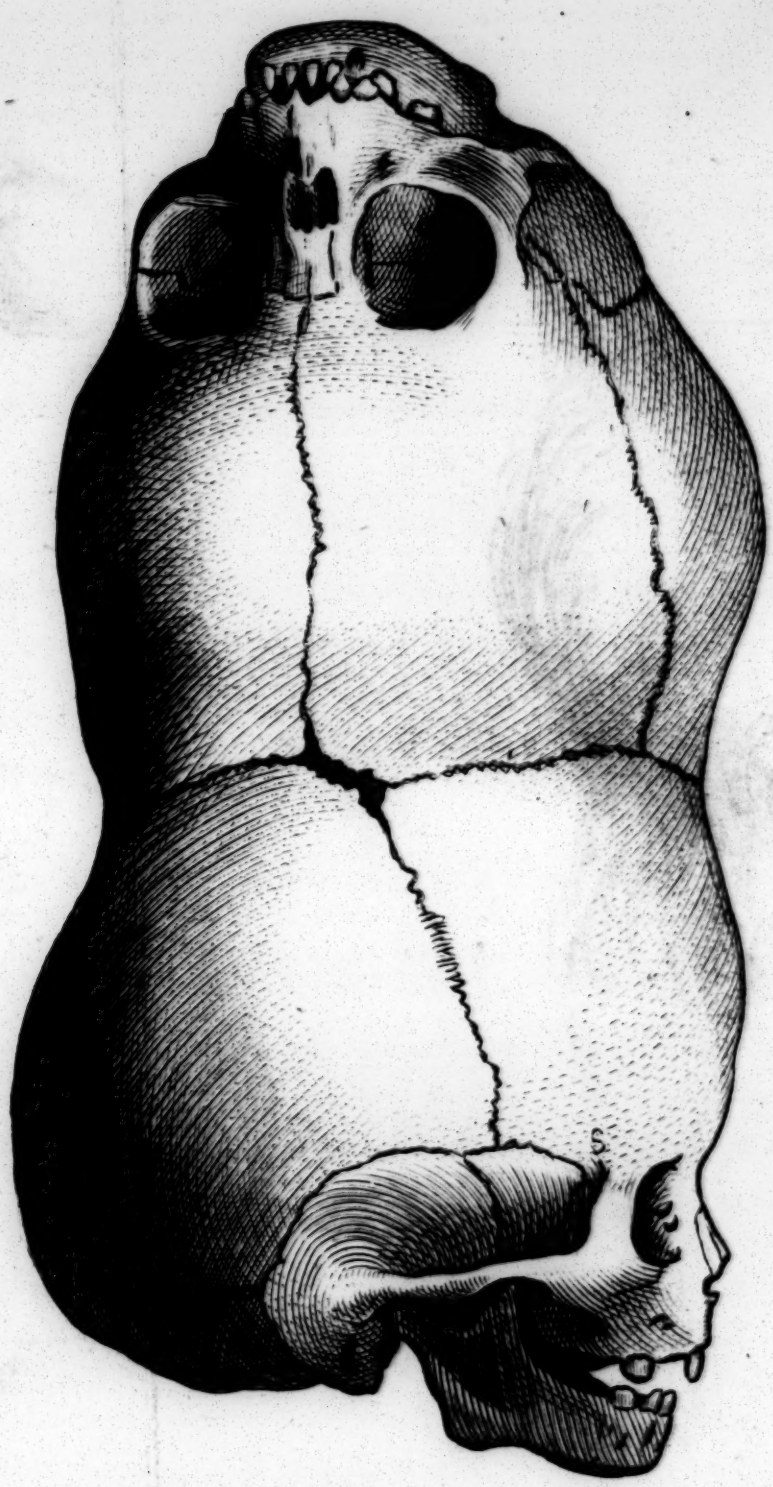


Fig. II.

Basire sc.

The ſuperficial veins upon the forehead of the upper head are very diſtinctly ſeen.

Fig. 2. An exact repreſentation of the double ſkull, which is now in Mr. HUNTER's collection, upon the ſame ſcale as fig. 1.

It ſhews the curious manner in which the two ſkulls are united together, and the number of teeth formed before the child's death; which circumſtance aſcertains, with tolerable accuracy, its age.

XVII. *On the Analyſis of a Mineral Subſtance from* New South Wales. *In a Letter from* Joſiah Wedgwood, *Eſq. F. R. S. and A. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read April 15, 1790.

DEAR SIR, Etruria, March 12, 1790.

I HAVE the pleaſure of acquainting you, that the clay from Sydney-Cove, which you did me the honour of ſubmitting to my examination, is an excellent material for pottery, and may certainly be made the baſis of a valuable manufacture for our infant colony there. Of the ſpecies of ware which may be produced from it, you will have ſome idea from the medallions I have ſent for your inſpection.

The other mineral, which you favoured me with a ſpecimen of, ſeems to me to contain one ſubſtance hitherto unknown, and another not known in the ſtate of purity in which it is here found. I ſhall therefore take the liberty of ſtating, as conciſely as I can, the principal reſults of the experiments that have been made upon it by myſelf and my aſſiſtant Mr. CHISOLM; ſubmitting it to your judgement, whether any of them be of importance enough to be laid before the Royal Society.

I have the honour to be, &c.

JOS. WEDGWOOD.

Analytical

Analytical experiments on a Mineral from Sydney-Cove *in* New South Wales.

THIS mineral is a mixture of fine white ſand, a ſoft white earth, ſome colourleſs micaceous particles, and a few black ones reſembling black mica or black-lead; partly looſe or detached from one another, and partly cohering together in little friable lumps.

None of theſe ſubſtances ſeem to be at all acted upon by the nitrous acid, concentrated or diluted; nor by oil of vitriol diluted with about equal its meaſure of water; in the cold, or in a boiling heat; the mineral remained unaltered in its appearance, and the acids had extracted nothing from it that could be precipitated by alkali.

Oil of vitriol boiled upon the mineral to dryneſs, as in the proceſs of making alum from clay, produced no apparent change in it; but a lixivium made from this dry maſs with water, on being ſaturated with alkali, became ſomewhat turbid, and depoſited, exceeding ſlowly, a white earth in a gelatinous ſtate, too ſmall in quantity for any particular examination; but which, from its aſpect, from the manner in which it was obtained, and from the taſte of the lixivium before the addition of the alkali, was judged to be the aluminous earth.

The marine acid, during digeſtion, ſeemed to have as little action as the other two; but on pouring in ſome water, with a view only to dilute and waſh out the remaining part of the acid, a remarkable difference preſented itſelf; the liquor became inſtantly white as milk, with a fine white curdly ſubſtance intermixed; the ſtrong acid having extracted ſomething which the ſimple dilution with water precipitated.

The white matter being waſhed off, more ſpirit of ſalt was added to the remainder, and the digeſtion repeated, with a long tube inſerted into the mouth of the glaſs, ſo as nearly to prevent evaporation. The acid, when cold and ſettled fine, was poured off clear; and on diluting it with water, the ſame milky appearance was produced as at firſt.

The digeſtion was repeated ſeveral times ſucceſſively, with freſh quantities of the acid, till no milkineſs appeared on dilution. The quantity of mineral employed was 24 grains; and the reſiduum, after the operations, waſhed and dried, weighed ſomewhat more than 19 grains; ſo that about one-fifth of it had been diſſolved. In ſome parcels of the mineral, taken up promiſcuouſly, the proportion of ſoluble matter was much leſs, and in none greater. It is only the white part, and only a portion of this, that the acid appears to act upon: the white ſand, much of the white ſoft earth, and all the black particles, remain unaltered.

To try whether this tedious proceſs of ſolution could be expedited by triture or calcination, ſome of the mineral was rubbed in a mortar; and in doing this, it appeared pretty remarkable, that though the black part bore but an inconſiderable proportion to the reſt, yet the whiteneſs of the other was ſoon covered and ſuppreſſed by it, the whole becoming an uniformly black, ſhining, ſoft, unctuous maſs, like black-lead rubbed in the ſame manner; with a few gritty particles perceptible on preſſing hard with the peſtle. A penny-weight of this mixt, ſpread thin on the bottom of a porcelain veſſel, was calcined about an hour, with a fire between 30 and 40 degrees *; it became of an uniform, dull, white, or grey colour, excepting

* By degrees of *fire*, or of heat *above ignition*, I mean thoſe of my thermometer;

excepting a very few, and very ſmall, ſparkling, black particles, ſuſpected to be thoſe which had eluded the action of the peſtle; it loſt in weight ſix grains, or one-fourth.

The mineral, thus ground and calcined, was found to be juſt as difficult of ſolution as in its crude ſtate; with this additional diſadvantage, that the undiſſolved fine particles are indiſpoſed to ſettle from the liquor.

In all the experiments of diſſolution, as often as the heat was at or near the boiling point of the acid, frequent, and pretty ſingular, burſts or exploſions happened, though the matter lay very thin in a broad-bottomed glaſs. They were ſometimes ſo conſiderable as to throw off a porcelain cup with which the glaſs was covered, and once to ſhatter the glaſs in pieces. In a heat a little below this, the extraction ſeemed to be equally complete, though more ſlow; but a heat a little below that in which wax melts, or below 140° of FAHRENHEIT's thermometer, appeared inſufficient.

To determine the degree of dilution neceſſary for the precipitation of the diſſolved ſubſtance, and whether the precipitation by water be total, a meaſure of the ſolution was poured into a large glaſs, and the ſame meaſure of water added repeatedly. The third addition of water occaſioned a ſlight milkineſs, which increaſed more and more to the ſixth. The liquor being then filtered off, another meaſure of water produced a little freſh milkineſs; and an eighth rather increaſed it; a ninth and a tenth had no effect. The liquor being now again paſſed through a filter, ſolution of ſalt of tartar did not in the leaſt alter its tranſparency; ſo that, after the ſolution

meter; and ſome idea may be formed of their value, by recollecting, that they commence at viſible redneſs; and that the extreme heat of a good air-furnace, of the common conſtruction, is 160°, or a little more.

has

has been diluted with eight or nine times its meaſure of water, there is nothing left in it that alkali can precipitate.

From the manner in which the ſolution is neceſſarily prepared, it cannot but contain a great redundance of acid; for the ſmall quantity of acid, ſufficient for holding the ſoluble part ſuſpended, would be ſoaked up or entangled by the undiſſolved part, ſo as ſcarcely to admit of any being poured off; and it cannot be diluted, or waſhed out, but by the ſtrong acid itſelf. The ſolution with which the above experiment was made was reckoned to have only about ſix grains of the ſoluble matter to three ounces of ſpirit of ſalt, having been prepared by digeſting that quantity of the ſpirit by half an ounce at a time on thirty grains of the crude mineral.

A *ſaturated* ſolution was obtained by digeſting, in a ſmall portion of the ſolutions thus prepared, the precipitate thrown down by water from the larger portions till the acid would take up no more. A ſolution, thus ſaturated, cannot bear the ſmalleſt quantity of water, a ſingle drop, on the firſt contact, producing a milky circle round it.

Examination of the above ſubſtance, extracted from the mineral by marine acid, and precipitated by water.

This ſubſtance, waſhed and dried, is indiſſoluble in water, as indeed might be expected from the manner of its preparation.

Nor is it acted upon by the nitrous or vitriolic acids, concentrated or diluted, cold or hot; nor by alkaline ſolutions, mild or cauſtic, of the volatile or fixed kind.

It is diſſolved by ſtrong marine acid, but not without the aſſiſtance of nearly the ſame degree of heat that is neceſſary for its extraction from the mineral. From this ſolution it is

precipitated by water; and, after repeated diſſolutions and precipitations, it appears to have ſuffered no decompoſition or change.

Spirit of nitre, added to the ſaturated ſolution, makes no precipitation; and if the quantity of nitrous acid exceeds, or at leaſt does not fall much ſhort of, that of marine acid in the ſolution, the mixture ſuffers no precipitation from water. Nor does any precipitation happen, though the nitrous ſpirit be *previouſly* mixed with even a large quantity of water; provided the quantity of ſolution added to it does not exceed that of the nitrous ſpirit in the mixture. The appropriate menſtruum for this ſubſtance (that is, for keeping it in a ſtate of dilute ſolution) appears therefore to be aqua regia; and the due proportions of the two acids, of any given ſtrength, might be determined, if neceſſary, with greater accuracy and facility for this than for any other body I know of; becauſe, if there be even a very minute ſurplus of marine acid in the ſolution, that ſurplus will inſtantly betray itſelf on dropping a little into water, all that was diſſolved by *it*, and no more, being precipitated by the water. It may be obſerved, however, that where an addition of nitrous acid is uſed, a *ſaturated* ſolution cannot be obtained (unleſs by ſubſequent evaporation), the ſame quantity of marine acid being neceſſary with as without that addition: the change, or modification, which the nitrous acid produces in the marine, ſerves, in the preſent inſtance, not for effecting the ſolution, as in the caſe of gold and ſome other metals, but merely for enabling it to bear water without depoſiting its contents.

Oil of vitriol, dropped into the ſaturated marine ſolution, occaſions no change till its quantity comes to be about equal to that of the ſolution; a conſiderable effervеſcence and heat are then

then produced, the liquor becomes milky, and the marine acid is extricated in its uſual white fumes. The mixture, heated nearly to boiling, becomes tranſparent, and afterwards continues ſo in the cold. This vitriolic ſolution is precipitated by water, and the precipitate is re-diſſolved by marine acid.

The ſaturated marine ſolution is indiſpoſed to cryſtallize. By continued evaporation in gentle heat, it becomes thick and butyraceous, and in this ſtate it ſoon liquefies again on expoſure to the air. The butyraceous maſs, in colour whitiſh or pale yellow, is not corroſive, like the ſimilar preparations made from ſome metallic bodies; nor is it more pungent in taſte, but rather leſs ſo than the combination of the ſame acid with calcareous earth. In a heat increaſed nearly to ignition, the acid is diſengaged, and riſes in white fumes, which, received in a cold phial, condenſe into colourleſs drops, without any appearance of ſublimate. From the remaining white maſs, ſpirit of nitre extracts ſo little as to exhibit only a ſlight milkineſs on adding alkali; a proof that nearly all the marine acid had been expelled; for, while that acid remains, the whole is diſſoluble by the nitrous.

The ſubſtance in queſtion is not precipitated by Pruſſian lixivium. A drop or two of the lixivium do indeed occaſion a little white or bluiſh-white precipitation in the *ſaturated* marine ſolution; but in the more dilute no turbidneſs appears till the quantity of lixivium is ſuch as to produce that effect by its mere water; and when the precipitate has at length been formed, it re-diſſolves in marine acid as eaſily as that made by water; whereas the precipitates reſulting from the union of the Pruſſian matter are not acted upon by acids, till that matter has been extracted from them by an alkali. For further ſatisfaction in this important point, the experiment was

repeated

repeated with a ſolution in aqua regia. Here the Pruſſian lixivium, in whatever quantity it was added, occaſioned no precipitation at all (only the uſual bluiſhneſs, ariſing from the iron always found in the common acids); and pure alkali, added afterwards, precipitated the original white ſubſtance unchanged.

The following experiments of precipitation by alkalies were made with the marine ſolution, before the effect of an addition of nitrous acid had been diſcovered; and they were made with ſo much care and attention, that it was not thought neceſſary to repeat them afterwards. To obviate, as much as poſſible, the equivocal reſults that might ariſe from *water* contained in the precipitants, the different alkalies were applied in the dryeſt ſtate I could reduce them to; *viz.* pure *ſalt of tartar*, kept for ſome time in a heat juſt below redneſs; cryſtals of *marine alkali*, melted and dried in the ſame manner; *volatile alkali* in cryſtals, a little ſurplus acid being, in this inſtance, previouſly added to the ſolution, to counteract the water of cryſtallization in the alkali; *ſalt of tartar cauſticated* by quicklime, and haſtily evaporated to dryneſs; the *marine alkali cauſticated* in like manner; and the vapour of *cauſtic volatile alkali* ariſing, with a very gentle heat, from a retort into a phial containing the ſolution. All theſe alkalies occaſioned copious precipitations. All the precipitates, after waſhing and drying, were found to re-diſſolve in marine acid; and, from all theſe ſolutions, the original ſubſtance was precipitated, unaltered, on diluting them with water.

In ſtrong fire, from 142 to 156 degrees, this ſubſtance diſcovers a much greater fuſibility than any of the known ſimple earths. In a ſmall veſſel, made of tobacco-pipe clay, it melted, and glazed the bottom; and on a bed of powdered flint, preſſed

ſmooth in the manner of a cupel, it did the ſame. Magneſia, or chalk, would indeed vitrify in the clay veſſel; but on flint, no one of the known earths ſhews any tendency to vitrification in that heat *. In a cavity, ſcooped in a lump of chalk, this ſubſtance, in the heat above mentioned, run into a ſmall round bead, ſmooth, whitiſh, and opaque, not in the leaſt adhering to the calcareous maſs. On a bed of powdered quicklime it formed a browniſh ſcoria, which in great part had ſunk into the lime, and ſeemed to have united with it. On Mr. HENRY's magneſia, uncalcined, it melted and ſunk in completely, leaving only a ſlight browniſh ſtain on the ſurface where it had lain. On beds of the baroſelenite and barytic quicklime, it likewiſe melted and ſunk in, leaving a diſcoloured ſpot behind; but whether it really united with the ſubſtrata, or only penetrated into their interſtices, could not be determined with certainty, on account of the ſmallneſs of the quantity of the mineral I had to work upon.

On a bed of powdered charcoal, in a crucible cloſely luted, this ſubſtance likewiſe melted; and therefore it may be pre-

* It may be proper juſt to mention, that I find this to be a very commodious and ſure method of trying, in ſmall, whether any given earthy body be fuſible with other earths. If the body is diſpoſed to vitrify with any proportion of clay or flint, for inſtance, it will equally vitrify when a little of it is applied, or even duſted only, on the bottom of a ſmall cup made of clay, or on a ſmooth cloſe bed of finely powdered flint. The body, in this mode of application, ſeems to unite with only juſt ſo much of the matter of the ſubſtratum as is requiſite for their moſt perfect fuſion together, and has nothing elſe in contact with it, ſo that no deception can ariſe; whereas, if *mixed* with the ſame matter, there might be no appearance of fuſion, unleſs certain favourable proportions of the two ſhould chance to be hit upon; and even then, if the quantity be ſmall, it would not be certain but that the fuſion might have originated from the matter of the crucible.

ſumed not to have owed its fuſion, in the above experiments, to the ſame cauſe to which ſome of the common ſimple earths, in certain circumſtances, owe theirs, namely, their union with the matter of the veſſel or ſupport, that is, with an earth or earths of a different kind from themſelves; but to poſſeſs a fuſibility ſtrictly its own, which takes place in a fire of 150 degrees, or perhaps leſs.

As charcoal in fine powder aſſumes a kind of fluidity in the fire, ſimilar to that which powdered gypſum exhibits in a ſmall heat, its ſurface had changed from concave to horizontal, and the bead had ſunk to the bottom; it was rough and black on the outſide, and whitiſh within. On repeating the experiment in a cavity ſcooped in a piece of charcoal, the reſult was a blackiſh bead like the former, only ſmooth on the outſide, with ſomething of metallic brightneſs, not unlike that of black-lead. Both beads were very light, and had a conſiderable cavity within. All the internal part was whitiſh, without the leaſt metallic aſpect; and the external gloſſy blackneſs appeared to be only the ſtain which charcoal powder communicates, in ſtrong fire, to ſome earthy bodies that have a tendency to vitrify. By boiling in concentrated marine acid a part of the beads was diſſolved, precipitable as at firſt by water; but an accident prevented the proceſs from being continued ſufficiently to determine whether the whole could be diſſolved or not.

By this fuſibility in the fire; ſolubility in one only of the common mineral acids, and parting with the acid in a heat below ignition; precipitability by water, and non-precipitability by Pruſſian lixivium; this ſubſtance is ſtrongly diſcriminated from all the known earths and metallic calces. And as it ſuffers no decompoſition from any of the alkalies, in any of

the uſual modes of application, I preſume, it cannot be conſidered as a combination of any of thoſe earths or calces with any of the known acids; for all the combinations of this kind would, in one or other of the above methods of trial, have had the earth or metal diſengaged from the acid.

Whether this ſubſtance belongs to the *earthy* or *metallic* claſs, I cannot abſolutely determine; but am inclined to refer it to the earthy; becauſe, though brought into perfect fuſion, in contact with inflammable matter, and in cloſe veſſels, it does not aſſume the appearance which metallic bodies do in that circumſtance.

Examination of the black particles.

Theſe particles, which bore but a very ſmall proportion to the other matter, were in form of ſhining black ſcales, very thin, and very light. One grain weight of them, carefully picked out, expoſed to a fire which was gradually raiſed to about 90°, and continued in all about 40 hours, in a veſſel looſely covered, was almoſt wholly diſſipated, and what little remained was perfectly white. Marine acid had no effect on it.

Fifteen grains of the entire mineral loſt, in the ſame fire, three grains. After ſeparating from another portion of the mineral, by waſhing and otherwiſe, a conſiderable quantity of the white matter, 15 grains of the remainder, containing of courſe more than its due proportion of the black, loſt five grains; ſo that it ſeems principally to be the ſubſtance on which the blackneſs depends that is deſtroyed or diſſipated by fire. The ſame quantity, 15 grains, of common black-lead loſt in the ſame fire above 14 grains, the reſiduum weighing leſs than one grain.

Though

Though no concluſion can be drawn from theſe experiments reſpecting the comparative *loſs* of black-lead and the *pure* black matter of this mineral, on account of the heterogeneous parts intermixed with the latter, the *colour* of the reſidua ſeems to afford a ſufficient diſcrimination between them; that of black-lead being dark reddiſh brown, but the others purely and uniformly white.

As this ſubſtance could not now be ſuppoſed to be either iron mica, or the common kind of black-lead, ſuſpicion fell upon molybdæna. I had not, at that time, had an opportunity of procuring a ſpecimen of molybdæna to compare it with; but from the ſingular and ſtrongly-marked properties of the molybdænic acid, diſcovered by SCHEELE, it was judged, that a very ſmall quantity of it, when diſengaged from the ſulphur with which it is naturally combined, would eaſily be diſtinguiſhable.

HJELM's proceſs for diſengaging the ſulphur, by repeatedly burning linſeed oil upon the molybdæna in a crucible, and afterwards abſtracting ſucceſſive quantities of the ſame oil from it in a retort, was tried on a portion of the Sydney-Cove mineral, from which much of the white matter had been ſeparated as above mentioned. The black coal, remaining in the retort, became yellow by calcination, as that of molybdæna ſhould do; but in this yellow powder, no veſtige of molybdænic acid could be diſcovered.

Another quantity of the mineral was ſubmitted to SCHEELE's own proceſs, *viz.* repeated abſtractions of *diluted* nitrous acid; but, inſtead of becoming whiter every time, and at length white as chalk, which molybdæna ſhould do, the blackneſs of this matter continued unaltered to the laſt.

There is one circumſtance in Mr. SCHEELE's experiments, which, though omitted by thoſe who have given abſtracts of them, may deſerve, on the preſent occaſion, to be more particularly noticed. He reduced the molybdæna into fine powder, and poured upon it *concentrated* nitrous acid: "the mixture," he ſays, "was hardly lukewarm in the retort, when it paſſed all "together into the recipient with great heat;" and it was for *this* reaſon that he afterwards uſed *diluted* acid. Preſuming that this violent action of the concentrated nitrous acid might afford a deciſive criterion of molybdæna, I had the black reſiduum, after five or ſix abſtractions of the diluted acid, ground fine upon a levigating glaſs, and returned into the retort, with ſix times its weight of ſmoking ſpirit of nitre. The heat was increaſed cautiouſly far beyond lukewarm, but no commotion could be perceived, except the exploſions already mentioned, which always took place when the mixture was near boiling. The diſtillation was continued to dryneſs, and repeated five times with the ſmoking acid; but the mineral remained juſt as black as it was at firſt.

Now, as SCHEELE's molybdæna is ſlowly decompoſed by the diluted nitrous acid, and rapidly acted upon by the concentrated acid, while the black part of this mineral obſtinately reſiſts both, I think we cannot heſitate to conclude, that this black ſubſtance is not SCHEELE's molybdæna. There are ſome other circumſtances which confirm this concluſion, although, taken ſingly, they would not, perhaps, be of much weight, conſidering the great proportion of other matter here mixed with the black. The principal of theſe circumſtances are, that it yields no flowers before a blow-pipe, and that its particles ſeem to have no flexibility or elaſticity, the only difficulty

of

of reducing it into fine powder ariſing from a property of another kind, unctuoſity.

The difference, above taken notice of, between this black matter and common black-lead, conſiſts only in the former leaving on calcination a white ſubſtance, ſeemingly ſiliceous, and the latter a brown ferrugineous one. In their aſpect, unctuoſity, reſiſtance to acids, and the volatility (in open fire) of that part in which the blackneſs conſiſts, they perfectly agree; and they appear to agree alſo in the nature or conſtitution of this volatile part; for the Sydney-Cove mineral, as well as black-lead, deflagrates and effervesces very ſtrongly with nitre, produces an hepatic impregnation on fuſion with vitriolated alkali, but none with pure alkali, and is manifeſtly rich in inflammable matter, without ſulphur.

It ſeems, therefore, that this ſubſtance is a *pure* ſpecies of plumbago, or black-lead, not taken notice of by any writer I have met with. Fourcroy, in the laſt edition of his Chemiſtry, looks upon iron as an eſſential component part of black-lead, to which, accordingly, he gives a new name expreſſive of that metal, *carbure de fer*. Lavoisier, in his Elements of Chemiſtry, lately publiſhed, mentions a *carbure* of zinc alſo, and ſays that both theſe carbures are called plumbago, or black-lead. The quantity of mineral I had been furniſhed with was too far exhauſted, before I met with this obſervation, to admit of any further experiments, for determining the preſence of zinc in it; but thoſe already ſtated, with the recollection of ſome circumſtances attending them, perſuade me, that *that* metallic body has no ſhare in its compoſition. Neither before the blow-pipe, nor in calcination, was there any appearance of the peculiar flame, or flowers, by which

zinc

zinc is ſo ſtrongly characteriſed: if any ſuch appearance had taken place, it could not have eſcaped notice, as ſome of the calcinations were particularly attended to during the proceſs, though with a different view, the diſcovery of ſulphur or arſenic. The white matter which remains after the calcination is certainly not calx of zinc, for it was not acted on by ſpirit of ſalt, cold or hot, while the calces of zinc are diſſolved rapidly by that acid, even in the cold.

XVIII. *Report on the beſt Method of proportioning the Exciſe upon Spirituous Liquors.* *By* Charles Blagden, *M. D. Sec. R. S. and F. A. S.*

Read April 22, 1790.

PART I.

On the Experiments.

IN conſequence of an application from Government to Sir Joseph Banks, Bart. Preſident of the Royal Society, for the beſt means of aſcertaining the juſt proportion of duty to be paid by any kind of ſpirituous liquor that ſhould come before the Officers of Exciſe, I was requeſted by that Gentleman to aſſiſt in planning the proper experiments for this purpoſe, and to draw up the Report upon them when they ſhould be finiſhed.

Though various indications of the ſtrength of ſpirituous liquors have been deviſed, applicable in a groſs manner to general uſe, it is well known that no method admits of real accuracy but that of the ſpecific gravity. The weights of an equal bulk of water and pure ſpirit differ from one another by at leaſt a ſixth part of the weight of the former; whence it is obvious, that, when thoſe two fluids are mixed together, the compound muſt have ſome intermediate ſpecific gravity, approaching nearer to that of water or pure ſpirit, as the former or the latter is the

more predominant ingredient. Were it not for a certain effect attending the mixture of water and ſpirits, which has been called their mutual penetration, the ſpecific gravity of theſe compoſitions, in a given degree of heat, would be ſimply in the arithmetical proportion of the quantity of each of the fluids entering into them. But whenever different ſubſtances, which have a ſtrong tendency to unite together, are mixed, the reſulting compound is found to occupy leſs ſpace than the ſubſtances forming it held in their ſeparate ſtate; wherefore the ſpecific gravity of ſuch compounds is always greater than would be given by a ſimple caculation from the volume of their ingredients. Though it be a general fact, that ſuch a decreaſe of bulk takes place on the mixture of ſubſtances which have a chemical attraction for each other, yet the quantity of this diminution is different in them all, and, under our preſent ignorance of the intimate compoſition of bodies, can be determined by experiment only. To aſcertain, therefore, the quantity and law of the condenſation reſulting from this mutual penetration of water and ſpirit, was the firſt object to which the following experiments were directed.

All bodies, in general, expand by heat; but the quantity of this expanſion, as well as the law of its progreſſion, are probably not the ſame in any two ſubſtances. In water and ſpirit they are remarkably different. The whole expanſion of pure ſpirit from 30° to 100° of FAHRENHEIT's thermometer, is not leſs than $\frac{1}{25}$th of its whole bulk at 30°; whereas that of water, in the ſame interval, is only $\frac{1}{145}$th of its bulk. The laws of their expanſion are ſtill more different than the quantities. If the expanſion of quickſilver be, as uſual, taken for the ſtandard (our thermometers being conſtructed with that fluid), the expanſion of ſpirit is, indeed, progreſſively increaſ-

ing with reſpect to that ſtandard, but not much ſo within the above-mentioned interval; whilſt water kept from freezing to 30°, which may eaſily be done, will abſolutely contract as it is heated for ten or more degrees, that is, to 40° or 42° of the thermometer, and will then begin to expand as its heat is augmented, at firſt ſlowly, and afterwards gradually more rapidly, ſo as to obſerve upon the whole a very increaſing progreſſion. Now, mixtures of theſe two ſubſtances will, as may be ſuppoſed, approach to the leſs or the greater of thoſe progreſſions, according as they are compounded of more ſpirit or more water, whilſt their total expanſion will be greater, according as more ſpirit enters into their compoſition; but the exact quantity of the expanſion, as well as law of the progreſſion, in all of them, can be determined only by trials. Theſe were, therefore, the two other principal objects to be aſcertained by experiment.

The firſt ſtep towards a right performance of the experiments was to procure the two ſubſtances with which they were to be made as pure as poſſible. Diſtilled water is in all caſes ſo nearly alike, that no difficulty occurred with regard to it; but the ſpecific gravity of pure ſpirit, or alcohol, has been given ſo very differently by the authors who have treated of it, that a particular ſet of experiments appeared neceſſary for determining to what degree of ſtrength rectified ſpirits could conveniently be brought. The perſon engaged to make theſe experiments was Dr. DOLLFUSS, an ingenious Swiſs gentleman then in London, who had diſtinguiſhed himſelf by ſeveral publications on chemical ſubjects. Dr. DOLLFUSS, having been furniſhed by Government with ſpirit for the purpoſe, rectified it by repeated and ſlow diſtillations till its ſpecific gravity became ſtationary in this manner of operating: he then

added dry cauſtic alkali to it, let it ſtand for a few days, poured off the liquor, and diſtilled it with a ſmall addition of burnt alum, placing the receiver in ice. By this method he obtained a ſpirit whoſe ſpecific gravity was ,8188 at 60° of heat. Perceiving, however, that he could not conveniently get the quantity of ſpirit he wanted lighter than ,82527 at 60°, he fixed upon that ſtrength as a ſtandard, to which he found the above-mentioned lighter ſpirit could be reduced by adding to it a $\frac{27}{100}$th part of water; and with this ſpirit and diſtilled water he made a ſeries of experiments for determining the ſpecific gravity of different mixtures of theſe fluids in different degrees of heat.

The proceſs followed by Dr. DOLLFUSS is not here given as the beſt poſſible for obtaining pure ſpirit; nor was the reſult of it, in fact, the lighteſt alcohol that has been procured. Some ſpirit has been tried ſince that time, whoſe ſpecific gravity was ,813 at 60°. This was furniſhed by Dr. GEORGE FORDYCE, F. R. S. who ſucceeded in bringing it to that ſtrength chiefly by adding the alkali very hot. Care muſt be taken that none of the cauſtic alkali comes over in the diſtillation. Some alcohol was alſo ſent, for trial, by Mr. LEWIS, an eminent diſtiller in Holborn, whoſe ſpecific gravity, at the ſame temperature, was ,814.

It was with ſpirit rectified from malt-ſpirits that Dr. DOLLFUSS's ſeries of experiments was made; but he tried ſeveral comparative experiments with ſuch as had been rectified from rum and brandy, and found no other difference than might fairly be aſcribed to unavoidable errors.

Upon examining the reſults of Dr. DOLLFUSS's experiments it was perceived, that though the numbers agreed together tolerably well upon the whole, yet in ſome places there was

that degree of irregularity in the firſt differences as made it adviſable to repeat ſeveral of the experiments; and Dr. Dollfuss leaving England about that time, the buſineſs of this repetition was intruſted to Mr. Gilpin, Clerk of the Royal Society. This gentleman had already taken a part in the buſineſs, by aſſiſting Dr. Dollfuss in the former experiments, particularly in the very nice part of weighing the mixtures; and his great ſkill, accuracy, and patience, in conducting experiments, as well as in computations, had on other occaſions been proved to many Members of the Society. One experiment leading on to another, Mr. Gilpin was at length induced to go through the whole ſeries anew; and as the deductions in this Report will be taken chiefly from that laſt ſet of experiments, it is proper here to deſcribe minutely the method obſerved by Mr. Gilpin in his operation. This naturally reſolves itſelf into two parts, the way of making the mixtures, and the way of aſcertaining their ſpecific gravity.

1. The mixtures were made by weight, as the only accurate method of fixing the proportions. In fluids of ſuch very unequal expanſions by heat as water and alcohol, if meaſures had been employed, increaſing or decreaſing in regular proportions to each other, the proportions of the maſſes would have been ſenſibly irregular; now the latter was the object in view, namely, to determine the real quantity of ſpirit in any given mixture, abſtracting the conſideration of its temperature. Beſides, if the proportions had been taken by meaſure, a different mixture ſhould have been made at every different degree of heat. But the principal conſideration was, that with a very nice balance, ſuch as was employed on this occaſion, quantities can be determined to much greater exactneſs by weight, than by any practicable way of meaſurement. The proportions were, therefore,

fore, always taken by weight. A phial being provided of ſuch a ſize as that it ſhould be nearly full with the mixture, was made perfectly clean and dry, and being counterpoiſed, as much of the pure ſpirit as appeared neceſſary was poured into it. The weight of this ſpirit was then aſcertained, and the weight of diſtilled water, required to make a mixture of the intended proportions, was calculated. This quantity of water was then added, with all the neceſſary care, the laſt portions being put in by means of a well known inſtrument, which is compoſed of a ſmall diſh terminating in a tube drawn to a fine point: the top of the diſh being covered with the thumb, the liquor in it is prevented from running out through the tube by the preſſure of the atmoſphere, but inſtantly begins to iſſue by drops, or a very ſmall ſtream, upon raiſing the thumb. Water being thus introduced into the phial, till it exactly counterpoiſed the weight, which, having been previouſly computed, was put into the oppoſite ſcale, the phial was ſhaken, and then well ſtopped with its glaſs ſtopple, over which leather was tied very tight, to prevent evaporation. No mixture was uſed till it had remained in the phial at leaſt a month, for the full penetration to have taken place; and it was always well ſhaken before it was poured out to have its ſpecific gravity tried.

2. There are two common methods of taking the ſpecific gravity of fluids; one by finding the weight which a ſolid body loſes by being immerſed in them; the other by filling a convenient veſſel with them, and aſcertaining the increaſe of weight it acquires. In both caſes a ſtandard muſt have been previouſly taken, which is uſually diſtilled water; namely, in the firſt method by finding the weight loſt by the ſolid body in the water, and in the ſecond method, the weight of the

veſſel

veſſel filled with water. The latter was preferred for the following reaſons.

When a ball of glaſs, which is the propereſt kind of ſolid body, is weighed in any ſpirituous or watery fluid, the adheſion of the fluid occaſions ſome inaccuracy, and renders the balance comparatively ſluggiſh. To what degree this effect proceeds is uncertain; but from ſome experiments made by Mr. GILPIN, with that view, it appears to be very ſenſible. Moreover, in this method a large ſurface muſt be expoſed to the air during the operation of weighing, which, eſpecially in the higher temperatures, would give occaſion to ſuch an evaporation as to alter eſſentially the ſtrength of the mixture. It ſeemed alſo, as if the temperature of the fluid under trial could be determined more exactly in the method of filling a veſſel, than in the other: for the fluid cannot well be ſtirred while the ball to be weighed remains immerſed in it; and as ſome time muſt neceſſarily be ſpent in the weighing, the change of heat which takes place during that period will be unequal through the maſs, and may occaſion a ſenſible error. It is true, on the other hand, that, in the method of filling a veſſel, the temperature could not be aſcertained with the utmoſt preciſion, becauſe the neck of the veſſel employed, containing about ten grains, was filled up to the mark with ſpirit not exactly of the ſame temperature, as will be explained preſently; but this error, it is ſuppoſed, would by no means equal the other, and the utmoſt quantity of it may be eſtimated very nearly. Finally, it was much eaſier to bring the fluid to any given temperature when it was in a veſſel to be weighed, than when it was to have a ſolid body weighed in it; becauſe in the former caſe the quantity was ſmaller, and the veſſel containing

ing it more manageable, being readily heated with the hand or warm water, and cooled with cold water: and the very circumſtance, that ſo much of the fluid was not required, proved a material convenience. The particular diſadvantage in the method of weighing in a veſſel, is the difficulty of filling it with extreme accuracy; but when the veſſel is judiciouſly and neatly marked, the error of filling will, with due care, be exceedingly minute. By ſeveral repetitions of the ſame experiments, Mr. GILPIN ſeemed to bring it within the $\frac{1}{15000}$th part of the whole weight.

The above-mentioned conſiderations induced me, as well as the gentlemen employed in the experiments, to give the preference to weighing the fluid itſelf; and that was accordingly the method practiſed both by Dr. DOLLFUSS and Mr. GILPIN in their operations.

The veſſel choſen as moſt convenient for the purpoſe was a hollow glaſs ball, terminating in a neck of a ſmall bore. That which Dr. DOLLFUSS uſed held 5800 grains of diſtilled water; but, as our balance was ſo extremely accurate, it was thought expedient, upon Mr. GILPIN's repetition of the experiments, to uſe one of only 2965 grains capacity, as admitting the heat of any fluid contained in it to be more nicely determined. The ball of this veſſel, which may be called the weighing-bottle, meaſured about 2,8 inches in diameter, and was ſpherical, except a ſlight flattening on the part oppoſite to the neck, which ſerved as a bottom for it to ſtand upon. Its neck was formed of a portion of a barometer tube, ,25 of an inch in bore, and about $1\frac{1}{2}$ inch long; it was perfectly cylindrical, and on its outſide, very near the middle of its length, a fine circle or ring was cut round it with a diamond, as the mark to which it was to be filled

filled with the liquor. This mark was made by fixing the bottle in a lathe, and turning it round with great care, in contact with the diamond. The glaſs of this bottle was not very thick; it weighed 916 grains, and, with its ſilver cap, 936.

When the ſpecific gravity of any liquor was to be taken by means of this bottle, the liquor was firſt brought nearly to the required temperature, and then the bottle was filled with it up to the beginning of the neck only, that there might be room for ſhaking it. A very fine and ſenſible thermometer (to be more particularly deſcribed hereafter) was then paſſed through the neck of the bottle into the contained liquor, which ſhewed whether it was above or below the intended temperature. In the former caſe the bottle was brought into colder air, or even plunged for a moment in cold water; the thermometer in the mean time being frequently put into the contained liquor, till it was found to ſink to the right point. In like manner when the liquor was too cold, the bottle was brought into warmer air, immerſed in warm water, or more commonly held between the hands, till upon repeated trials with the thermometer the juſt temperature was found. It will be underſtood, that during the courſe of this heating or cooling, the bottle was very frequently ſhaken between each immerſion of the thermometer; and the top of the neck was kept covered, either with the finger, or a ſilver cap made on purpoſe, as conſtantly as poſſible. Hot water was uſed to raiſe the temperature only in heats of 80° and upwards, inferior heats being obtained by applying the hands to the bottle; when the hot water was employed, the ball of the bottle was plunged into it and again quickly lifted out, with the neceſſary ſhaking interpoſed, as

often as was neceſſary for communicating the required heat to the liquor; but care was taken to wipe the bottle dry after each immerſion, before it was ſhaken, leſt any adhering moiſture might by accident get into it. The liquor having by theſe means been brought to the deſired temperature, the next operation was to fill up the bottle exactly to the mark upon the neck, which was done with ſome of the ſame liquor, by means of a glaſs funnel with a very ſmall bore. Mr. GILPIN endeavoured to get that portion of the liquor which was employed for this purpoſe, pretty nearly to the temperature of the liquor contained in the bottle; but as the whole quantity to be added never exceeded ten grains, a difference of ten degrees in the heat of that ſmall quantity, which is more than it ever amounted to, would have occaſioned an error of only $\frac{1}{30}$ of a degree in the temperature of the maſs. Enough of the liquor was put in, to fill the neck rather above the mark, and the ſuperfluous quantity was then abſorbed to great nicety, by bringing into contact with it the fine point of a ſmall roll of blotting paper. As the ſurface of the liquor in the neck would be always concave, the bottom or centre of this concavity was the part made to coincide with the mark round the glaſs: and in viewing it care was taken, that the near and oppoſite ſides of the mark ſhould appear exactly in the ſame line, by which means all parallax was avoided. A ſilver cap, which fitted tight, was then put upon the neck, to prevent evaporation; and the whole apparatus was in that ſtate laid in the ſcale of the balance, to be weighed with all the exactneſs poſſible.

The ſpirit employed by Mr. GILPIN was furniſhed to him by Dr. DOLLFUSS, under whoſe inſpection it had been rectified from rum ſupplied by Government. Its ſpecific gravity, at 60

degrees of heat, was ,82514. It was firſt weighed pure, in the above-mentioned bottle, at every five degrees of heat, from 30 to 100 incluſively. Then mixtures were formed of it and diſtilled water, in every proportion from $\frac{1}{20}$th of the water to equal parts of water and ſpirit; the quantity of water added being ſucceſſively augmented, in the proportion of five grains to one hundred of the ſpirit; and theſe mixtures were alſo weighed in the bottle, like the pure ſpirit, at every 5 degrees of heat. The numbers hence reſulting are delivered in the following table; where the firſt column ſhews the degrees of heat; the ſecond gives the weight of the pure ſpirit contained in the bottle at thoſe different degrees; the third gives the weight of a mixture in the proportions of 100 parts by weight of that ſpirit to 5 of water, and ſo on ſucceſſively till the water and the ſpirit are in equal parts. The bottle itſelf, with its cap, having been previouſly counterpoiſed, theſe numbers are the weights of the liquor contained in it, in grains and hundredths of a grain. They are the mean of three ſeveral experiments at leaſt, as Mr. GILPIN always filled and weighed the bottle over again that number of times, if not oftener. The heat was taken at the even degree, as ſhewn by the thermometer, without any allowance in the firſt inſtance, becauſe the coincidence of the mercury with a diviſion can be perceived more accurately than any fraction can be eſtimated; and the errors of the thermometers, if any, it was ſuppoſed would be leſs upon the grand diviſions of 5 degrees, than in any others. It muſt be obſerved, that Mr. GILPIN uſed the ſame mixture throughout all the different temperatures, heating it up from 30° to 100°; hence ſome ſmall error

in its ſtrength may have been occaſioned, in the higher degrees, by more ſpirit evaporating than water; but this, it is believed, muſt have been trifling, and greater inconvenience would probably have reſulted from interpoſing a freſh mixture.

TABLE

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I.

temperature.

grains irit to grains ater.	100 grains of ſpirit to 35 grains of water.	100 grains of ſpirit to 40 grains of water.	100 grains of ſpirit to 45 grains of water.	100 grains of ſpirit to 50 grains of water.
ns.	Grains.	Grains.	Grains	Grains.
6,16	2653,54	2669,64	2684,63	2698,41
9,77	2647,30	2663,48	2678;43	2692.32
3,42	2641,02	2657,35	2672,37	2686,37
7,04	2634,68	2650,96	2666,13	2680,25
0,59	2628,26	2644,68	2659,95	2674,04
4,07	2621,77	2638,25	2653,55	2667,72
7,50	2615,26	2631,82	2647,20	2661,45
0,80	2608,72	2625,41	2640,80	2655,09
4,23	2602,14	2618,89	2634,30	2648,65
7,47	2595,43	2612,20	2627,78	2642,17
0,52	2588,61	2605,32	2621,03	2635,47
3,80	2581,91	2598,76	2614,48	2628,87
6,95	2575,20	2592,17	2607 86	2622,30
9,95	2568,18	2585,12	2601,12	2615,70
3,08	2561,28	2578,37	2594,45	2609,11

grains irit to grains ater.	100 grains of ſpirit to 90 grains of water.	100 grains of ſpirit to 95 grains of water.	100 grains of ſpirit to 100 grains of water.
ns.	Grains.	Grains.	Grains.
1,26	2779,21	2786,47	2793,26
5,47	2773,53	2780,75	2787,59
9,85	2767,78	2775,15	2782,06
4,13	2762,03	2769,55	2776,40
8,22	2756,25	2763,67	2770,62
2,25	2750,32	2757,82	2764,72
6,26	2744,32	2751,87	2758,82
0,27	2738,35	2745,93	2752,82
4,22	2732,42	2740,00	2746,88
7,95	2726,25	2733,92	2740,83
1,50	2719,78	2727,49	2734,49
5,37	2713,69	2721,47	2728,60
9,10	2707,44	2715,22	2722,32
2,81	2701,18	2708,91	2716,04
6,36	2694.76	2702,50	2709,75

In

TABL

Weights at the different d

Heat.	The pure ſpirit.	100 grains of ſpirit to 5 grains. of water.	100 grains of ſpirit to 10 grains of water.	100 grains of ſpirit to 15 grains of water.	100 grains of ſpirit to 20 grains of water.	100 of ſ 25 of
°	Grains.	Grains.	Grains.	Grains.	Grains.	Gra
30	2487,32	2519,98	2548,59	2573,86	2596,65	26
35	2480,79	2513,48	2541,96	2567,34	2590,15	26
40	2474,18	2506,98	2535,52	2560,83	2583,70	26
45	2467,52	2500,33	2528,90	2554,24	2577,16	25
50	2460,77	2493,48	2522,10	2547,61	2570,64	25
55	2453,84	2486,51	2515,30	2540,88	2563,94	25
60	2446,86	2479,75	2508,60	2534,19	2557,23	25
65	2440,04	2472,97	2501,87	2527,51	2550,56	25
70	2433,37	2466,28	2495,00	2520,65	2543,84	25
75	2426,47	2459,18	2488,03	2513,63	2536,91	25
80	2419,18	2451,95	2480,83	2506,61	2529,85	25
85	2412,02	2444,80	2473,68	2499,59	2523,08	25
90	2404,92	2437,72	2466,64	2492,62	2516,20	25
95	2397,75	2430,56	2459,51	2485,51	2509,15	25
100	2390,64	2423,53	2452,63	2478,59	2502,15	25

Heat.	100 grains of ſpirit to 55 grains of water.	100 grains of ſpirit to 60 grains of water.	100 grains of ſpirit to 65 grains of water.	100 grains of ſpirit to 70 grains of water.	100 grains of ſpirit to 75 grains of water.	100 of ſ 80 of
°	Grains.	Grains.	Grains.	Grains.	Grains.	Gr
30	2711,19	2723,00	2733,84	2744,19	2753,67	27
35	2705,08	2716,96	2727,78	2738,24	2747,90	27
40	2699,09	2711,02	2721,90	2732,45	2742,18	27
45	2692,97	2704,82	2715,98	2726,38	2736,21	27
50	2686,81	2698,63	2709,92	2720,37	2730,27	27
55	2680,57	2692,44	2703,67	2714,27	2724,20	27
60	2674,31	2686,16	2697,44	2708,18	2718,26	27
65	2667,97	2680,00	2691,22	2701,99	2712,06	27
70	2661,67	2673,68	2685,02	2695,66	2705,87	27
75	2655,19	2667,32	2678,60	2689,34	2699,57	27
80	2648,47	2660,69	2672,02	2682,77	2693,03	27
85	2641,85	2654,19	2665,54	2676,40	2686,77	26
90	2635,38	2647,61	2659,07	2670,09	2680,60	26
95	2628,83	2641,10	2652,56	2663,64	2674,20	26
100	2622,22	2634,38	2645,95	2657,14	2667,61	26

[To face p. 332.

A B L E I.

ifferent degrees of temperature.

0 grains ſpirit to grains water.	100 grains of ſpirit to 25 grains of water.	100 grains of ſpirit to 30 grains of water.	100 grains of ſpirit to 35 grains of water.	100 grains of ſpirit to 40 grains of water.	100 grains of ſpirit to 45 grains of water.	100 grains of ſpirit to 50 grains of water.
ains.	Grains.	Grains.	Grains.	Grains.	Grains	Grains.
596,65	2617,24	2636,16	2653,54	2669,64	2684,63	2698,41
590,15	2610,80	2629,77	2647,30	2663,48	2678;43	2692.32
583,70	2604,50	2623,42	2641,02	2657,35	2672,37	2686,37
577,16	2597,99	2617,04	2634,68	2650,96	2666,13	2680,25
570,64	2591,50	2610,59	2628,26	2644,68	2659,95	2674,04
563,94	2584,79	2604,07	2621,77	2638,25	2653,55	2667,72
557,23	2578,22	2597,50	2615,26	2631,82	2647,20	2661,45
550,56	2571,48	2590,80	2608,72	2625,41	2640,80	2655,09
543,84	2564,89	2584,23	2602,14	2618,89	2634,30	2648,65
536,91	2558,14	2577,47	2595,43	2612,20	2627,78	2642,17
529,85	2551,10	2570,52	2588,61	2605,32	2621,03	2635,47
523,08	2544,41	2563,80	2581,91	2598,76	2614,48	2628,87
516,20	2537,57	2556,95	2575,20	2592,17	2607 86	2622,30
509,15	2530,51	2549,95	2568,18	2585,12	2601,12	2615,70
502,15	2523,59	2543,08	2561,28	2578,37	2594,45	2609,11

0 grains ſpirit to 5 grains water.	100 grains of ſpirit to 80 grains of water.	100 grains of ſpirit to 85 grains of water.	100 grains of ſpirit to 90 grains of water.	100 grains of ſpirit to 95 grains of water.	100 grains of ſpirit to 100 grains of water.
rains.	Grains.	Grains.	Grains.	Grains.	Grains.
753,67	2762,61	2771,26	2779,21	2786,47	2793,26
747,90	2756,97	2765,47	2773,53	2780,75	2787,59
742,18	2751,35	2759,85	2767,78	2775,15	2782,06
736,21	2745,47	2754,13	2762,03	2769,55	2776,40
730,27	2739,52	2748,22	2756,25	2763,67	2770,62
724,20	2733,47	2742,25	2750,32	2757,82	2764,72
718,26	2727,56	2736,26	2744,32	2751,87	2758,82
712,06	2721,47	2730,27	2738,35	2745,93	2752,82
705,87	2715,40	2724,22	2732,42	2740,00	2746,88
699,57	2709,08	2717,95	2726,25	2733,92	2740,83
693,03	2702,57	2711,50	2719,78	2727,49	2734,49
686,77	2696,33	2705,37	2713,69	2721,47	2728,60
680,60	2690,22	2699,10	2707,44	2715,22	2722,32
674,20	2683,79	2692,81	2701,18	2708,91	2716,04
667,61	2677,25	2686,36	2694.76	2702,50	2709,75

In

In order to deduce the ſpecific gravities from the numbers in the preceding table, it was neceſſary to weigh diſtilled water in the ſame veſſel. This Mr. GILPIN did, in the ſame manner as before, at the different degrees of heat; and the reſult of his experiments is delivered in the following table, where the firſt column ſhews the heat, and the ſecond gives the weight of the water, at that temperature, contained in the bottle.

TABLE II.

Weights and ſpecific Gravities of diſtilled Water.

Heat.	Weight of the water.	Specific gravity of the water.
°	Grains.	
30		
35	2967,03	1,00087
40	2967,34	1,00091
45	2967,29	1,00084
50	2966,97	1,00066
55	2966,39	1,00040
60	2965,39	1,00000
65	2964,17	,99952
70	2962,72	,99896
75	2961,03	,99832
80	2959,13	,99762
85	2957,03	,99685
90	2954,80	,99602
95	2952,20	,99507
100	2949,36	,99404

There

There would be two methods of computing the ſpecific gravity, at the different temperatures, from theſe numbers; one, by taking the weight of the water, at the particular temperature in queſtion, for the ſtandard; and the other by fixing on one certain temperature of the water, for inſtance 60°, to be the ſtandard, with its bulk at which that of the ſpirit at all different degrees ſhall be compared. I have preferred the latter method, though not the moſt uſual, becauſe it ſhews, more readily and ſimply, the progreſſion obſerved in the changes of ſpecific gravity, according to the heat and ſtrength of the mixture. This method, however, rendered it neceſſary to make an allowance for the contraction and expanſion of the bottle uſed for weighing the liquors, according to the deviation of their temperature from 60°, either below or above. To obtain this correction, the expanſion of hollow glaſs was taken from General ROY's experiments in the LXXVth volume of the Philoſophical Tranſactions, as ,0000517 of an inch upon a foot for every degree of heat, whence its effect, in enlarging the capacity of a ſphere, was computed, and the reſulting correction added to the weight of the liquors in heats below 60°, and ſubtracted from it in heats above. On the ſame account a third column is given, in the preceding table, to ſhew the ſpecific gravity of water at the different temperatures, its weight at 60° being taken as the ſtandard.

Another correction alſo became neceſſary, on account of the part of the ſtem of the thermometer which was not immerſed in the liquor. This inſtrument, made by RAMSDEN, had its ball ,22 of an inch in diameter, and its ſtem 13 inches in length. From the ball to the commencement of the ſcale 3,6 inches of the ſtem were bare, and then the ſcale began, which

reached

reached from 15 to 110 degrees. The part of it particularly made uſe of in theſe experiments, namely, from 30° to 100°, meaſured 6,82 inches. The ſcale was made of ivory, and carried diviſions to every fifth of a degree, the quarters of which could be readily eſtimated; ſo that the inſtrument could be read off to twentieths of degrees. When the thermometer was immerſed in the weighing-bottle, the liquor reached up nearly to what would have been 0° upon its ſtem; hence, as the heat of the room in which the experiments were made remained about 60°, the correction for the different heat of the quickſilver in the ſtem from that in the ball of the thermometer was calculated according to Mr. CAVENDISH's table, given in the LXVIIth volume of the Philoſophical Tranſactions. Thus the real heat of the fluid in the weighing-bottle being found, an allowance was made to reduce it to the exact degree indicated on the ſcale of the thermometer.

The preciſe ſpecific gravity of the pure ſpirit employed was ,82514; but to avoid an inconvenient fraction it is taken, in conſtructing the table of ſpecific gravities, as ,825 only, a proportionable deduction being made from all the other numbers. Thus the following table gives the true ſpecific gravity, at the different degrees of heat, of a pure rectified ſpirit, whoſe ſpecific gravity at 60° is ,825, together with the ſpecific gravities of different mixtures of it with water, at thoſe different temperatures, as far as equal parts by weight.

TABLE

TABLE III.

Real ſpecific gravities at the different temperatures.

Heat.	The pure ſpirit.	100 grains of ſpirit to 5 grains of water.	100 grains of ſpirit to 10 grains of water.	100 grains of ſpirit to 15 grains of water.	100 grains of ſpirit to 20 grains of water.	100 grains of ſpirit to 25 grains of water.	100 grains of ſpirit to 30 grains of water.	100 grains of ſpirit to 35 grains of water.	100 grains of ſpirit to 40 grains of water.	100 grains of ſpirit to 45 grains of water.	100 grains of ſpirit to 50 grains of water.
30°	,83899	,85001	,85967	,86819	,87589	,88284	,88922	,89509	,90053	,90558	,91023
35	,83673	,84776	,85737	,86592	,87363	,88061	,88700	,89292	,89839	,90342	,90811
40	,83445	,84551	,85515	,86367	,87140	,87843	,88481	,89074	,89626	,90133	,90605
45	,83215	,84321	,85286	,86140	,86913	,87617	,88260	,88855	,89405	,89916	,90393
50	,82981	,84084	,85051	,85910	,86688	,87392	,88036	,88632	,89187	,89702	,90177
55	,82741	,83843	,84815	,85677	,86455	,87159	,87809	,88406	,88963	,89479	,89957
60	,82500	,83609	,84583	,85445	,86223	,86931	,87582	,88181	,88740	,89259	,89741
65	,82262	,83374	,84350	,85213	,85991	,86698	,87352	,87954	,88518	,89037	,89518
70	,82032	,83142	,84111	,84975	,85758	,86469	,87121	,87725	,88291	,88810	,89294
75	,81792	,82896	,83869	,84731	,85517	,86234	,86886	,87491	,88058	,88585	,89068
80	,81543	,82649	,83623	,84491	,85276	,85993	,86648	,87258	,87822	,88352	,88839
85	,81291	,82396	,83371	,84243	,85036	,85757	,86411	,87021	,87590	,88120	,88605
90	,81044	,82150	,83126	,84001	,84797	,85518	,86172	,86787	,87360	,87889	,88376
95	,80794	,81900	,82877	,83753	,84550	,85272	,85928	,86542	,87114	,87654	,88146
100	,80548	,81657	,82639	,83513	,84308	,85031	,85688	,86302	,86879	,87421	,87915

Heat.	100 grains of ſpirit to 55 grains of water.	100 grains of ſpirit to 60 grains of water.	100 grains of ſpirit to 65 grains of water.	100 grains of ſpirit to 70 grains of water.	100 grains of ſpirit to 75 grains of water.	100 grains of ſpirit to 80 grains of water.	100 grains of ſpirit to 85 grains of water.	100 grains of ſpirit to 90 grains of water.	100 grains of ſpirit to 95 grains of water.	100 grains of ſpirit to 100 grains of water.
30°	,91454	,91853	,92219	,92568	,92888	,93191	,93483	,93751	,93996	,94225
35	,91242	,91644	,92009	,92362	,92687	,92995	,93281	,93553	,93796	,94027
40	,91034	,91438	,91805	,92161	,92489	,92799	,93086	,93353	,93602	,93835
45	,90822	,91222	,91599	,91950	,92281	,92595	,92887	,93153	,93407	,93638
50	,90608	,91007	,91388	,91740	,92075	,92388	,92681	,92952	,93202	,93436
55	,90390	,90791	,91170	,91528	,91863	,92176	,92472	,92744	,92997	,93230
60	,90173	,90570	,90954	,91316	,91656	,91971	,92264	,92536	,92791	,93025
65	,89952	,90359	,90738	,91100	,91440	,91769	,92055	,92328	,92584	,92816
70	,89733	,90136	,90522	,90880	,91225	,91547	,91845	,92121	,92377	,92608
75	,89507	,89916	,90298	,90660	,91005	,91326	,91625	,91909	,92164	,92397
80	,89277	,89690	,90072	,90435	,90780	,91103	,91404	,91683	,91943	,92179
85	,89043	,89460	,89843	,90209	,90558	,90882	,91186	,91465	,91729	,91969
90	,88817	,89230	,89617	,89988	,90342	,90668	,90967	,91248	,91511	,91751
95	,88588	,89003	,89390	,89763	,90119	,90443	,90747	,91029	,91290	,91531
100	,88357	,88769	,89158	,89536	,89889	,90215	,90522	,90805	,91066	,91310

From

From this table, when the ſpecific gravity of any ſpirituous liquor is aſcertained, it will be eaſy to find the quantity of rectified ſpirit, of the above-mentioned ſtandard, contained in any given quantity of it, either by weight or meaſure. As common arithmetic is competent to furniſh the rules for this purpoſe, it would be ſuperfluous to give them here. All the objects of inquiry relative to this buſineſs ſhould, I think, be reduced to Tables; the firſt of which might exhibit the ſpecific gravities of different mixtures, from one to 100 parts of water, increaſing by one, at every degree of heat from 40 to 80, being the utmoſt limits of temperature that can be wanted in common practice. This table need only be calculated to three places of figures, which will always give the quantity of ſpirit true within a fiftieth part of the whole, and in the moſt uſual degrees of heat within a hundredth; and to this number of figures the areometer, or hydrometer, ſhewing the ſpecific gravities, could be ſuited. A further reaſon for continuing only to three places of figures is, that, accurate as Mr. GILPIN's experiments have been, ſome irregularities are found in the two laſt of the five decimals to which his tables are calculated. The greateſt of theſe irregularities, I think, do not exceed the quantity correſponding to a difference of one-fifth of a degree of heat, and in general they are much leſs. A table might be conſtructed to ſhew what the numbers would probably have been, to the five places of decimals, if there had been no kind of error in the experiments.—Another table ſhould be of the volumes, exhibiting what proportion the ſpirit and water bore to each other by meaſure or bulk, in the different mixtures; whence might be calculated a very uſeful table of Diminutions, to ſhew when a given weight, or volume, of a certain ſpirit and wa-

ter are mixed together, how much their bulk would be diminiſhed; or, what is called by the diſtillers the *concentration*. From ſuch a table the diſtiller could learn, what quantity of water he muſt mix with ſpirit of a given ſtrength, in order to reduce it to proof ſpirit, or any other ſtrength; and likewiſe what quantity of proof ſpirit, or ſpirit of any other ſtrength, he may obtain, by adding water to ſpirit of a given ſtrength; both circumſtances very neceſſary to be known in the trade, and which ſome of the ſliding rulers now in uſe profeſs to point out.

It may appear odd, that no mention has been made till now of *proof ſpirit*, the ſtandard to which moſt of the regulations of the exciſe have hitherto been referred. The reaſons for not adopting this ſtandard are: firſt, that the ſtrength of ſpirit to be called proof is a mere arbitrary point, and by no means ſo exactly determined as could be wiſhed; and, ſecondly, that it ſeemed moſt convenient to take for the ſtandard the higheſt ſtrength of ſpirit uſually found in commerce, and beyond which it cannot be rectified without a proceſs of ſome expence, ſo that all the other degrees of ſtrength might be reckoned one way, without the intervention of a middle point, inducing the neceſſity of denominating ſome *above* and others *under*. If, however, Government ſhould find it expedient to preſerve the reference to proof ſpirit, from the tables given in this Report others may be conſtructed, in which all the old terms of over and under proof ſhould be retained, and have a preciſe meaning, as ſoon as the ſtrength to be called proof ſhall be finally ſettled. By the Act of 2 Geo. III. it is ordered, that the gallon of brandy or ſpirits of the ſtrength of one to ſix under proof, ſhall be taken and reckoned at 7 *lb.* 13 *oz.*, which is underſtood by the trade to mean at 55° of heat. Hence,

taking the weight of a gallon of water at the ſame heat to be 8 *lb.* 5,66 &c. *oz.* *, the ſpecific gravity of this diluted ſpirit will be found ,9335 at 60° †; whence, by a computation founded on the tables in this Report, the ſpecific gravity of proof ſpirit will come out ,916. But the rulers of correction belonging to DICAS's and QUIN's hydrometers give the ſpecific gravity of proof ſpirits about ,922 at 55°, equivalent to ,920 at 60°. The former, ,916, correſponds to a mixture of 100 parts of ſpirit with 62 by meaſure, or 75 by weight, of water; and the latter, ,920, to a mixture of 100 parts of ſpirit and 66 by meaſure, or 80 by weight, of water. The difference is conſiderable; but the firſt is undoubtedly moſt conformable to the exiſting Acts of Parliament. If, therefore, it be thought right to preſerve the term proof-ſpirit in our Exciſe Laws, it may be underſtood to mean ſpirit, whoſe ſpecific gravity is ,916, and which is compoſed of 100 parts of rectified ſpirit at ,825, and 62 parts of water by meaſure, or 75 by weight; the whole at 60 degrees of heat.

I have choſen this point of the thermometer, 60°, in preference to 55°, becauſe it is much the moſt ſuitable for experiments, being the temperature at which a room feels pleaſant, and in which any operation, however ſlow and tedious, can be executed without the uneaſy ſenſation of cold: for this reaſon it has been adopted by many Engliſh philoſophers. In the table formerly recommended, from 40 to 80 degrees of the thermometer, it will be the middle temperature.

* Probably 8 *lb.* 5,72 *oz.* is nearer.

† This ſpecific gravity indicates a mixture of 107 grains of water with 100 of ſpirit, and conſequently is below Mr. GILPIN's preſent Tables, which go only to equal parts.

The ſpecific gravity of ,825 having been fixed upon as the ſtandard of rectified ſpirit in our tables, Mr. GILPIN was deſired to aſcertain by experiment what proportion of water would be neceſſary, to reduce the lighteſt alcohol in his poſſeſſion to that ſtandard. This was ſome alcohol, already mentioned, which Mr. LEWIS had furniſhed; and its ſpecific gravity being ,814196 at 60°, 3000 grains of it mixed with 135 grains of diſtilled water formed a compound, whoſe ſpecific gravity was ,825153; that is, in round numbers, 100 grains of alcohol at ,814 with 4,5 grains of water, form our ſtandard of ſpirit at ,825.

Perhaps ſome perſons may object to the preceding experiments, on account of the ſmall quantities mixed and weighed. Undoubtedly experiments on a large ſcale have ſome advantages; but theſe in general depend more on the coarſeneſs of tools, and clumſineſs of operators, than on the nature of the operations. If inſtruments be exceedingly exact, and the experimenter equal to the taſk of uſing them properly, I believe the errors upon moderate quantities will be quite as ſmall in proportion as upon large; and in this particular inſtance, where the greateſt ſource of error lay in the determination of the heat, the ſmaller quantities had in that reſpect an evident advantage, it being much eaſier to bring ſix ounces of a liquor to an uniform temperature than ſo many gallons. One of our moſt eſſential inſtruments, namely, the balance, was ſo much ſuperior in nicety to any thing that could be wanted in theſe experiments, that error in weighing muſt be thrown entirely out of the queſtion. It was conſtructed by Mr. RAMSDEN; and ſome account, though very imperfect, of its admirable mechaniſm, as well as of its extreme ſenſibility, even when

loaded with conſiderable weights, has been given in the XXXIIId volume of the *Journal de Phyſique*.

I muſt not conclude this part of the Report without obſerving, that as the experiments were made with pure ſpirit and water, if any extraneous ſubſtances are contained in the liquor to be tried, the ſpecific gravity in the tables will not give exactly the proportions of water and ſpirit in it. The ſubſtances likely to be found in ſpirituous liquors, where no fraud is ſuſpected, are, eſſential oils, ſometimes empyreumatic, mucilaginous or extractive matter, and perhaps ſome ſaccharine matter. The effect of theſe, in the courſe of trade, ſeems to be hardly ſuch as would be worth the cognizance of the exciſe, nor could it eaſily be reduced to any certain rules. Eſſential and empyreumatic oils are nearly of the ſame ſpecific gravity as ſpirit, in general rather lighter, and therefore, notwithſtanding the mutual penetration, will probably make little change in the ſpecific gravity of any ſpirituous liquor in which they are diſſolved. The other ſubſtances are all heavier than ſpirit; the ſpecific gravity of common gum being 1,482, and of ſugar 1,606, according to the tables of M. Brisson. The effect of them therefore will be to make ſpirituous liquors appear leſs ſtrong than they really are. An idea was once entertained of endeavouring to determine this matter with ſome preciſion; and accordingly Dr. Dollfuss evaporated 1000 grains of brandy, and the ſame quantity of rum, to dryneſs; the former left a reſiduum of 40 grains, the latter only of 8½ grains. The 40 grains of reſiduum from the brandy, diſſolved again in a mixture of 100 of ſpirit with 50 of water, increaſed its ſpecific gravity ,00041; hence the effect of this extraneous matter upon the ſpecific gravity of the brandy containing it would be to increaſe the fifth decimal by 6 nearly,

equal

equal to what would indicate, in the above-mentioned mixture, about one-ſeventh of a grain of water more than the truth, to 100 of ſpirit: a quantity much too minute for the conſideration of Government.

PART II.

On Hydrometers.

THE readieſt way of aſcertaining ſpecific gravities, and undoubtedly the moſt convenient for public buſineſs, is by hydrometers; and, I conceive, thoſe of the ſimpleſt conſtruction to be beſt upon the whole, eſpecially if more accurate means are kept at hand, to be reſorted to in caſe of diſputes. An hydrometer of glaſs would be the moſt certain; but whether it be of that ſubſtance, or of metal, it ſhould conſiſt of a ball, or rather bulb, ſo poiſed as that a certain part ſhould be always downmoſt in the liquor, and having a ſtem riſing from it on the oppoſite part, which would conſequently keep upright in uſing the inſtrument. On the ſize of this ſtem, the ſenſibility of the hydrometer chiefly depends. In the old areometers the ſtem was made ſo large, that the volume of water diſplaced between its leaſt and greateſt immerſions was equal to the whole difference of ſpecific gravity between water and alcohol, or perhaps more; whence its ſcale of diviſions muſt be very ſmall, and could not give the ſpecific gravity with much accuracy. To remedy this defect, weights were introduced, by means of which the ſtem could be made ſmaller, each weight affording a new commencement of its ſcale; ſo that the ſize of the diviſions on a given length of ſtem was doubled, tripled, quadrupled,

quadrupled, &c. according as one, two, three, or more weights were employed, the diameter of the ſtem being leſſened in the ſubduplicate proportion of the increaſed length of the diviſions. Of late this principle ſeems to have been carried to exceſs; the number of weights adapted to ſome hydrometers being ſo great as to prove very inconvenient in practice. A mean between the two methods would certainly be beſt, which might be ſuited to our tables in the following manner.

It is propoſed to determine the ſpecific gravity to three places of decimals, water being taken as unity: the whole compaſs of numbers, therefore, from rectified ſpirit to water, at 60 degrees of heat, would be the difference between ,825 and 1,000, that is, 175; call it 220 to include the lighteſt ſpirit and heavieſt water, at all the common temperatures. Of theſe diviſions the ſtem might give every twenty, and then ten weights would be ſufficient for the whole 220. By making the ſtem carry twenty diviſions, an inconvenience much complained of, that of ſhifting the weights, would in great meaſure be avoided; becauſe a perſon converſant in ſuch buſineſs would ſeldom err to that extent in judging of the ſtrength of his ſpirit previous to trial; and yet the ſtem would not need to be ſo large, or the diviſions ſo ſmall, as to preclude the deſired accuracy. In conformity to this arrangement it would be proper, that the weights adapted to the hydrometer ſhould be marked with the numbers of the ſpecific gravity, zero on the top of its ſtem, without a weight, being ſuppoſed to mean 800, and 20 at the bottom of the ſtem to ſignify 820, which number the firſt weight would carry; the ſucceſſive weights would be marked 840, 860, &c.; and the diviſion on the ſtem cut by the fluid under trial would be a number to be always added to the number marked upon the weight, the ſum of the two

ſhewing

ſhewing the true ſpecific gravity. The weights ſhould unqueſtionably be made to apply on the top of the ſtem, ſo as never to come into contact with the liquor; and in uſing the hydrometer its ſtem ſhould always be preſſed down lower than the point at which it will ultimately reſt, that by being wetted it may occaſion no reſiſtance to the fluid. The inſtrument itſelf ſhould be of as regular a ſhape, and with as few inequalities and protuberances, as poſſible, that all unneceſſary obſtruction to its motions may be avoided.

As it is not probable but diſputes will ſometimes ariſe, I think it would be adviſable, that ſome of the principal exciſe offices ſhould be provided with a good pair of ſcales, and a weighing-bottle properly marked, the quantity of whoſe contents of diſtilled water at 60° had been previouſly determined. By filling this bottle up to the mark with the ſpirit in queſtion, and dividing its increaſe of weight by the given weight of water required to fill it, the ſpecific gravity of the ſpirit would, I think, be better aſcertained, even under the management of a common operator, than by the moſt dexterous uſe of the hydrometer.

The ſimpleſt and moſt equitable method of levying the duty on ſpirituous liquors would be, to conſider rectified ſpirit as the true and only exciſable matter. On this principle, all ſuch liquors would pay exactly according to the quantity of rectified ſpirit they contain; ſo that when a caſk, for inſtance, of any ſpirits was preſented to the revenue officer, his buſineſs would be to determine from the quantity, ſpecific gravity, and temperature, of the liquor, how many gallons, or pounds, of rectified ſpirit enter into its compoſition; each of which gallons, or pounds, ſhould be charged a certain ſum. The complicated regulations attending the adaption of the duties to

different

different degrees of ſtrength would thus be avoided; and it is believed, that many frauds might be prevented, which artful perſons have now an opportunity of practiſing, by altering the ſtrength of their ſpirit in a variety of ways. From the tables already recommended, it would be eaſy to deduce this quantity of rectified ſpirit, either by weight or meaſure, in any given quantity of a ſpirituous liquor; or other tables might be conſtructed which ſhould ſhew it at once by inſpection.

If, however, it be thought by Government moſt expedient not to make any eſſential change in the preſent manner of collecting this article of the revenue, I would at leaſt recommend, that the ſpecific gravity ſhould be ſubſtituted for the relation to proof ſpirit. Thus, inſtead of ordering ſo much duty *per* gallon to be paid by ſpirits one to fix under proof, it may be enacted, that the ſame ſum ſhall be paid by ſpirit of ,9335 ſpecific gravity, or, not to be too preciſe, by ſpirit from ,930 to ,935, and ſo on for any other degrees of ſtrength; a certain temperature, ſuppoſe 60°, being always underſtood to be meant when ſpecific gravity is mentioned in an Act of Parliament.

The duties to be laid according to either of theſe methods may readily be adjuſted or equalized to thoſe paid at preſent, as far as the latter can be determined from the Act of 2 George III. referred to above, or by any of the inſtruments now in uſe.

It will give me very ſincere pleaſure if the preceding experiment and remarks ſhould tend to advance the proſperity of this country, by introducing a more juſt and advantageous mode of collection into ſo material a branch of the revenue.

XIX. *Obſervations on the Sugar Ants. In a Letter from* John Caſtles, *Eſq. to Lieut. Gen.* Melvill, *F. R. S.*

Read May 22, 1790.

THE Sugar Ants, ſo called from their ruinous effects on the ſugar-cane, firſt made their appearance in Grenada about twenty years ago on a ſugar plantation at Petit Havre, a Bay five or ſix miles from the town of St. George, the capital, conveniently ſituated for ſmuggling from Martinique. It was therefore concluded, they were brought from thence in ſome veſſel employed in that trade; which is very probable, as colonies of them in like manner were afterwards propagated in different parts of the iſland by droghers, or veſſels employed in carrying ſtores, &c. from one part of the iſland to another.

From thence they continued to extend themſelves on all ſides, for ſeveral years; deſtroying in ſucceſſion every ſugar plantation between St. George's and St. John's, a ſpace of about twelve miles. At the ſame time, colonies of them began to be obſerved in different parts of the iſland, particularly at Duqueſne on the north, and Calavini on the ſouth ſide of it.

All attempts of the planters to put a ſtop to the ravages of theſe inſects having been found ineffectual, it well became the legiſlature to offer great public rewards to any who ſhould diſcover a practicable method of deſtroying them, ſo as to permit the cultivation of the ſugar-cane as formerly. Accordingly,

an Act was paſſed, by which ſuch diſcoverer was entitled to twenty thouſand pounds, to be paid from the public treaſury of the iſland.

Many were the candidates on this occaſion, but very far were any of them from having any juſt claim: nevertheleſs, conſiderable ſums of money were granted, in conſideration of trouble and expences in making experiments.

In Grenada there had always been ſeveral ſpecies of ants, differing in ſize, colour, &c. which however were perfectly innocent with reſpect to the ſugar-cane. The ants in queſtion, on the contrary, were not only highly injurious to it, but to ſeveral ſorts of trees, ſuch as the lime, lemon, orange, &c.

Theſe ants are of the middle ſize, of a ſlender make, of a dark red colour, and remarkable for the quickneſs of their motions; but their greateſt peculiarities were, their taſte when applied to the tongue, the immenſity of their number, and their choice of places for their neſts.

All the other ſpecies of ants in Grenada have a bitter muſky taſte. Theſe, on the contrary, are acid in the higheſt degree, and, when a number of them were rubbed together between the palms of the hands, they emitted a ſtrong vitriolic ſulphureous ſmell; ſo much ſo, that, when this experiment was made, a gentleman conceived, that it might be owing to this quality that theſe inſects were ſo unfriendly to vegetation. This criterion to diſtinguiſh them was infallible, and known to every one.

Their numbers were incredible. I have ſeen the roads coloured by them for miles together; and ſo crowded were they in many places, that the print of the horſes feet would appear for a moment or two, till filled up by the ſurrounding multitude. This is no exaggeration. All the other ſpecies of ants,

although numerous, were circumſcribed and confined to a ſmall ſpot, in proportion to the ſpace occupied by the cane ants, as a mole hill to a mountain.

The common black ants of that country had their neſts about the foundation of houſes or old walls; others in hollow trees; and a large ſpecies, in the paſtures, deſcending by a ſmall aperture under ground. The ſugar ants, I believe, univerſally conſtructed their neſts among the roots of particular plants and trees, ſuch as the ſugar-cane, lime, lemon, and orange trees, &c.

The deſtruction of theſe ants was attempted chiefly two ways; by poiſon and the application of fire.

For the firſt purpoſe arſenic and corroſive ſublimate, mixed with animal ſubſtances, ſuch as ſalt fiſh, herrings, crabs, and other ſhell-fiſh, &c, were uſed, which was greedily devoured by them. Myriads of them were thus deſtroyed; and the more ſo, as it was obſerved by a magnifying glaſs, and indeed (though not ſo diſtinctly) by the naked eye, that corroſive ſublimate had the effect of rendering them ſo outrageous that they deſtroyed each other; and that effect was produced even by coming into contact with it. But it is clear, and it was found, that theſe poiſons could not be laid in ſufficient quantities over ſo large a tract of land, as to give the hundred-thouſandth part of them a taſte, and conſequently they proved inadequate to the taſk.

The uſe of fire afforded a greater probability of ſucceſs; for (from whatever cauſe) it was obſerved, that if wood, burnt to the ſtate of charcoal, without flame, and immediately taken from the fire, was laid in their way, they crowded to it in ſuch amazing numbers as ſoon to extinguiſh it, although with the deſtruction of thouſands of them in effecting it. This part of

their

their hiſtory appears ſcarcely credible; but, on making the experiment myſelf, I found it literally true. I laid fire, as above deſcribed, where there appeared but a very few ants, and in the courſe of a few minutes thouſands were ſeen crowding to it and upon it, till it was perfectly covered by their dead bodies. Holes were therefore dug at proper diſtances in a cane piece, and fire made in each of them. Prodigious quantities periſhed in this way; for thoſe fires, when extinguiſhed, appeared in the ſhape of mole hills, from the numbers of their dead bodies heaped on them. Nevertheleſs they ſoon appeared again as numerous as ever. This may be accounted for, not only from their amazing fecundity, but that probably none of the breeding ants, or young brood, ſuffered from the experiment.

For the ſame reaſon, the momentary general application of fire by burning the cane traſh (or ſtraw of the cane) as it lay on the ground, proved as little effectual; for although, perhaps, multitudes of ants might have been deſtroyed, yet in general they would eſcape by retiring to their neſts under cover, and out of its reach, and the breeding ants, with their young progeny, muſt have remained unhurt.

Mr. SMEATHMAN (who wrote a Paper on the Termites, or White Ants, of Africa, and was at Grenada at this time) imagined, that theſe ants were not the cauſe of the injury done to the canes. He ſuppoſed, it was owing to the blaſt, a diſeaſe the canes are ſubject to, ſaid to ariſe from a ſpecies of ſmall flies, generated on their ſtems and leaves; and that the ants were attracted in ſuch multitudes merely to feed on them.

There is no doubt, that where this blaſt exiſted it conſtituted part of the food of the ants: but this theory was overthrown, by obſerving, that by far the greateſt part of the injured canes had no appearance of that ſort, but became ſickly and withered,

withered, apparently for want of nouriſhment. Beſides, had that been the caſe, the canes muſt have been benefited inſtead of being hurt by theſe inſects.

For the cure of the blaſt, he propoſed the application of train oil, which had not the leaſt effect in preventing the miſchief, and, if it had, could never have been generally enough uſed to anſwer the purpoſe.

This calamity, which reſiſted ſo long the efforts of the planters, was at length removed by another, which, however ruinous to the other iſlands in the Weſt Indies, and in other reſpects, was to Grenada a very great bleſſing, namely, the hurricane in 1780; without which it is probable the cultivation of the ſugar-cane in the moſt valuable parts of that iſland muſt have in a great meaſure been thrown aſide, at leaſt for ſome years. How this hurricane produced this effect has been conſidered rather as a matter of wonder and ſurprize than attempted to be explained. By attending to the following obſervations, the difficulty I believe will be removed.

Theſe ants make their neſts, or cells for the reception of their eggs, only under or among the roots of ſuch trees or plants as are not only capable of protecting them from heavy rains, but are at the ſame time ſo firm in the ground as to afford a ſecure baſis to ſupport them againſt any injury occaſioned by the agitation of the uſual winds. This double qualification the ſugar-cane poſſeſſes in a very great degree; for a ſtool of canes (which is the aſſemblage of its numerous roots where the ſtems begin to ſhoot out) is almoſt impenetrable to rain, and is alſo, from the amazing numbers and extenſion of the roots, firmly fixed to the ground. Thus, when every other part of the field is drenched with rain, the ground under thoſe ſtools will be found quite dry, as I and every other planter muſt have

obſerved

obſerved when digging out the ſtools in a cane piece, to prepare for replanting. And when canes are lodged or laid down by the uſual winds, or from their own luxuriancy, the ſtools commonly remain in the ground; hence, in ordinary weather, their neſts are in a ſtate of perfect ſecurity.

The lime, lemon, orange, and ſome other trees, afford theſe inſects the ſame advantages, from the great number and quality of their roots, which are firmly fixed to the earth, and are very large; beſides which, their tops are ſo very thick and umbrageous as to prevent even a very heavy rain from reaching the ground underneath.

On the contrary, theſe ants neſts are never found at the roots of trees or plants incapable of affording the above protection: ſuch, for inſtance, is the coffee-tree. It is indeed ſufficiently firm in the ground, but it has only one large tap root, which goes ſtraight downwards, and its lateral roots are ſo ſmall as to afford no ſhelter againſt rain. So again, the roots of the cotton ſhrub run too near the ſurface of the earth to prevent the acceſs of rain, and are neither ſufficiently permanent, nor firm enough to reſiſt the agitation by the uſual winds. The ſame obſervation will be found true with reſpect to cocao, plantains, maize, tobacco, indigo, and many other ſpecies of trees and plants.

Trees or plants of the firſt deſcription always ſuffer more or leſs in lands infeſted with theſe ants; whereas thoſe of the latter never do. Hence we may fairly conclude, that the miſchief done by theſe inſects is occaſioned only by their lodging and making their neſts about the roots of particular trees or plants. Thus the roots of the ſugar-canes are ſomehow or other ſo much injured by them, as to be incapable of performing their office of ſupplying due nouriſhment to the plants,

which

which therefore become ſickly and ſtinted, and conſequently do not afford juices fit for making ſugar in either tolerable quantity or quality.

That theſe ants do not feed on any part of the canes or trees affected ſeems very clear, for no loſs of ſubſtance in either the one or the other has ever been obſerved; nor have they ever been ſeen carrying off vegetable ſubſtances of any ſort. The truth of this will farther appear by the following fact.

A very fine lime-tree, in the paſture of Mount William eſtate, at a conſiderable diſtance from any canes, but near the dwelling houſe, had ſickened and died ſoon after the ants made their appearance on that eſtate. After it had remained in that ſtate, without a ſingle leaf, or the leaſt verdure, for ſeveral months, on examination, a very few ants appeared about it; but when with the manager's permiſſion it was grubbed out, a moſt aſtoniſhing quantity of ants and ants neſts, full of eggs, were found about its roots, all of which were quite dead, and many of them rotten.

That this tree conſtituted no part of their food is quite certain; but, while it continued to afford them proper ſecurity for their neſts, they ſtill continued their abode.

On the contrary, there is the greateſt preſumption that theſe ants are carnivorous, and feed entirely on animal ſubſtances; for if a dead inſect, or animal food of any ſort, was laid in their way, it was immediately carried off. It was found almoſt impoſſible to preſerve cold victuals from them. The largeſt carcaſſes, as ſoon as they began to become putrid, ſo as that they could ſeparate the parts, ſoon diſappeared. Negroes with ſores had difficulty to keep the ants from the edges of them. They deſtroyed all other vermin, rats in particular, of which they cleared every plantation they came upon, which they probably

effected

effected by attacking their young. It was found that poultry, or other ſmall ſtock, could be raiſed with the greateſt difficulty; and the eyes, noſe, and other emunctories of the bodies of dying or dead animals were inſtantly covered with them.

In the year 1780, many of the ſugar eſtates which had been firſt infeſted with theſe ants had been either abandoned, or put into other kinds of produce, principally cotton; which, as I have above obſerved, do not afford conveniency for their neſts. In conſequence, the ants had there ſo much decreaſed in number, that the cultivation of ſugar had again begun to be re-aſſumed. But it was very different in thoſe plantations which had but lately been attacked, and were ſtill in ſugar. At Duqueſne, particularly, at that time they were pernicious in the higheſt degree, ſpreading themſelves on all ſides with great rapidity, when a ſudden ſtop was put to their progreſs by the hurricane which happened near the middle of October that year. How this was effected, I think, may be explained by attending to the above obſervations.

From what has been ſaid it appears, that a dry ſituation, ſo as to exclude the ordinary rains from their neſts or cells, appropriated for the reception of their eggs or young brood, is abſolutely neceſſary; but that theſe ſituations, however well calculated for the uſual weather, could not afford this protection from rain during the hurricane, may be eaſily conceived.

When by the violence of the tempeſt heavy pieces of artillery were removed from their places, and houſes and ſugar-works levelled with the ground, there can be no doubt that trees and every thing growing above ground muſt have greatly ſuffered. This was the caſe. Great numbers of trees and plants (which reſiſt commonly the ordinary winds) were torn out by the root. The canes were univerſally either lodged or

twiſted about as if by a whirlwind, or torn out of the ground altogether. In the latter caſe, the breeding ants, with their progeny, muſt have been expoſed to inevitable deſtruction from the deluge of rain which fell at the ſame time. The number of canes, however, thus torn out of the ground, could not have been adequate to the ſudden diminution of the ſugar ants; but it is eaſy to conceive, that the roots of canes which remained on the ground, and the earth about them, were ſo agitated and ſhaken, and at the ſame time the ants neſts were ſo broken open, or injured, by the violence of the wind, as to admit the torrents of rain accompanying it. I apprehend, therefore, that the principal deſtruction of theſe ants muſt have been thus effected.

Two circumſtances tended to facilitate this happy effect. Many of the roots of the canes infected, as above obſerved, were either dead or rotten, ſo as not to be capable of making the ſame reſiſtance to the wind as thoſe in perfect health. And this hurricane happened ſo very late as the month of October, when the canes are always ſo high above ground as to give the wind ſufficient hold of them, which at an earlier period would not have been the caſe.

That many of the cane ants were ſwept off by the torrents of rain into the rivers and ravines, and thus periſhed, I have no doubt; but if we conſider the obſtacles to this being very general, it could have had but ſmall effect in conſiderably reducing their numbers; for on flat land it could not have happened. In hanging or hilly land, the cane traſh would afford great ſhelter, and the ants would naturally retire to their neſts for ſecurity, when they found their danger.

Some have ſuppoſed, that the ſugar ants, after a certain time, degenerate, and become inoffenſive; and in proof of this, they

they ſay, Martinique and Barbadoes were freed from their bad effects without a hurricane or any other apparent cauſe.

The idea of any ſuch extraordinary and unheard-of deviation of nature is too contemptible to deſerve an anſwer; but the reaſon is obvious. The planters there either abandoned their cane lands, or planted them in coffee, cocao, cotton, indigo, &c. none of which, according to the above obſervations, afford the ants proper conveniency for the propagation of their ſpecies; and therefore their numbers muſt have ſo much decreaſed as to re-admit the culture of the ſugar-cane as before. At the ſame time it is very probable, that this diminution might have in part been owing to ſomething of the hurricane kind; for it is well known, that ſtrong ſqualls of wind, attended with heavy rains, are frequent in the Weſt Indies, although they do not laſt ſo long, nor are ſo violent, as to deſerve the name of a hurricane.

It muſt not however be denied, that though nature, for a time, may permit a particular ſpecies of animal to become ſo unproportionably numerous as to endanger ſome other parts of her works, ſhe herſelf will in due time put a check upon the too great increaſe; and that is often done by an increaſe of ſome other animal inimical to the former deſtroyers. In the preſent caſe, however, nothing of that ſort appeared; therefore, when a plain natural cauſe, obvious to our ſenſes, occurred, by which we can account for the amazing and ſudden decreaſe of thoſe ruinous inſects, it is unneceſſary to recur to other poſſible cauſes, too minute for our inveſtigation.

All I have ſaid on this ſubject would certainly be of little or no conſequence, did it not lead to the true method of cultivating the ſugar-cane on lands infeſted with thoſe deſtructive

inſects; in which point of view, however, it becomes important.

If then the above doctrine be juſt, it follows, that the whole of our attention muſt be turned to the deſtruction of the neſts of theſe ants, and conſequently the breeding ants with their eggs or young brood.

In order to effect this, all trees * and fences, under the roots of which theſe ants commonly take their reſidence, ſhould firſt be grubbed out; particularly lime fences, which are very common in Grenada, and which generally ſuffered from the ants before the canes appeared in the leaſt injured. After which the canes ſhould be ſtumped out with care, and the ſtools burnt as ſoon as poſſible, together with the field traſh (or the dried leaves and tops of the canes), in order to prevent the ants from making their eſcape to new quarters. The beſt way of doing this, I apprehend, will be, to gather the field traſh together in conſiderable heaps, and to throw the ſtools as ſoon as dug out of the ground into them, and immediately apply fire. By this means multitudes muſt be deſtroyed; for the field traſh, when dry, burns with great rapidity. The land ſhould then be ploughed or hoe-ploughed twice (but at leaſt once) in the wetteſt ſeaſon of the year, in order to admit the rains, before it is hoed for planting the cane: by theſe means theſe inſects, I apprehend, will be ſo much reduced in number as at leaſt to ſecure a good plant cane.

But it is the cuſtom in moſt of the Weſt India iſlands to permit the canes to rattoon; that is, after the canes have once been cut down, for the purpoſe of making ſugar, they are

* Particular fruit trees may probably be preſerved, without detriment, by carefully removing the earth from about their roots, deſtroying the ants neſts, and afterwards replacing either the ſame or new earth.

ſuffered

ſuffered to grow up again, without replanting; and this generally for three or four years, but ſometimes for ten, fifteen, or twenty. In this mode of culture the ſtools become larger every year, ſo as to grow out of the ground to a conſiderable height, and by that means afford more and more ſhelter to the ants neſts; therefore, for two or three ſucceſſive crops, the canes ſhould be replanted yearly, ſo as not only to afford as little cover as poſſible for the ants neſts, but continually to diſturb ſuch ants as may have eſcaped, in the buſineſs of propagating their ſpecies.

That conſiderable expence and labour will attend putting this method into execution, there is no doubt. An expenſive cure, however, is better than none; but from the general principles of agriculture, I am of opinion, that the planter will be amply repaid for his trouble, by the goodneſs of his crops, in conſequence of the ſuperior tilth the land will receive in the propoſed method. Of this we have a proof in the iſland of St. Kitt's, where they conſtantly replant their canes yearly: and it is very well known, that an acre of cane land there gives a greater return than the ſame quantity in any other iſland. In St. Kitt's, five hogſheads *per* acre is common yielding in good land. In Grenada, from two to three hogſheads from plant canes, and half that quantity from rattoons. Thus, although the St. Kitt's planter cuts only one half of his cane land yearly, in a given number of years he makes a greater revenue than the Grenada planter on the preſent mode of rattooning, when four-fifths of the cane land is yearly cut.

Some may be of opinion, that it would be more advantageous to change the produce than to purſue the propoſed method; on which I ſhall only obſerve, that it appears to me, that one half of the uſual crop of ſugar, thus produced,

will

will be more advantageous to the planter (when at the ſame time progreſs is making in deſtroying the ſugar ants) than a full crop of any other produce. In ſome very few ſituations cotton perhaps may be excepted. As to coffee, it is to be conſidered that it gives no return till the third year after planting, and not a full crop till the fifth. Cocao begins to bear in five years; but yields little till the ſeventh: and indigo not only exceedingly impoveriſhes the land, but is unhealthy to the negroes. Add to this, that far the greateſt part of ſugar lands are unfit for the culture of any of theſe.

It would carry this Letter to too great a length were I to adduce all that may be ſaid on this ſubject; I ſhall therefore conclude by obſerving, that the beſt proof of the truth of the above doctrine will be the ſucceſs attending the propoſed method of cultivation, or one of the ſame tendency, *viz.* to attempt the deſtruction of the neſts of theſe inſects, and conſequently the breeding ants, with their young broods; for their fecundity appears to be ſo prodigiouſly great as to render it altogether impoſſible to deſtroy them by poiſon, which can never be generally enough uſed to effect that purpoſe.

XX. *Experiments and Obſervations on the Diſſolution of Metals in Acids, and their Precipitations; with an Account of a new compound acid Menſtruum, uſeful in ſome technical Operations of parting Metals.* By James Keir, *Eſq.* F. R. S.

Read May 20, 1790.

IN the following Paper, I intend to relate two ſets of experiments; *one*, ſhewing the effects of *compounding the vitriolic and nitrous acids in diſſolving metals*; and the other, deſcribing ſome curious *appearances which occur in the precipitation of ſilver from its ſolution in nitrous acid by iron, and by ſome other ſubſtances.* In a ſubſequent Paper I hope to continue the ſubject of metallic diſſolution * and precipitation, firſt, by adding ſome *experiments on the quantities and kinds of gas* produced by diſſolving different metals in different acids, under various circumſtances; ſecondly, by ſubmitting certain *general propoſitions*,

* The Engliſh word *ſolution* has two ſignifications in chemiſtry; one, expreſſive of the act of diſſolving, as when we ſay, that "ſolution is a chemical "operation;" and the other, denoting the ſubſtance diſſolved in its ſolvent, as "a ſolution of ſilver in nitrous acid." The French language is equally equivocal, as the word "diſſolution" is uſed in both the above-mentioned ſenſes. In treating on this ſubject, in which both meanings were very frequently required, ſometimes in the ſame ſentence, I could not but be ſenſible of confuſion in the ſtyle, and I have therefore confined the word *ſolution* to expreſs the ſubſtance diſſolved together with its ſolvent, and the word *diſſolution* to denote the act of diſſolving.

which ſeem deducible from the facts related; and, laſtly, by concluding with ſome *reflexions* relative to the *theory of metallic diſſolution and precipitation.*

PART I.

On the effects of compounding the vitriolic and nitrous acids, under various circumſtances, upon the diſſolution of metals.

SECTION FIRST.

On the mixture of oil of vitriol and nitre.

1. The properties of the ſeveral acids, in their ſeparate ſtates, have been inveſtigated with conſiderable induſtry and ſucceſs; and thoſe of one compound, *aqua regis*, are well known on account of its frequent uſe in diſſolving gold: yet not only various other combinations of different acids remain to be examined; but alſo the changes of properties to which theſe mixed acids are ſubject, from the difference of circumſtances, eſpecially thoſe of *concentration*, *temperature*, and of that quality which is called, properly or improperly, *phlogiſtication*, are ſubjects ſtill open for enquiry.

2. As I ſhall have frequent occaſion to ſpeak of the *phlogiſtication* and *dephlogiſtication* of acids, I wiſh to premiſe, that by theſe terms I mean only certain *ſtates* or *qualities* of thoſe bodies, but without any *theoretical* reference. Thus vitriolic acid may be ſaid to be phlogiſticated by addition of ſulphur or other inflammable matter, by which it is converted into ſulphureous acid, without determining whether this change be

cauſed by the addition of the ſuppoſed principle *phlogiſton*, as one ſet of philoſophers believe, or by the action of the added inflammable ſubſtance in drawing from the acid a portion of its aërial principle, by which the ſulphur, its other element, is made to predominate, as others have lately maintained. It were much to be wiſhed, that we had words totally unconnected with theory; that chemiſts, who differ from each other in ſome ſpeculative points, may yet ſpeak the ſame language, and may relate their facts and obſervations, without having our attention continually drawn aſide from theſe to the different modes of explanation which have been imagined. But at preſent we have only the choice of terms between words derived from the ancient theory, and thoſe which have been lately propoſed by the oppoſers of that theory. In this dilemma I have preferred the uſe of the former, not that I wiſh to ſhew any predilection to either theory, but becauſe that ſyſtem, having long been generally adopted, is underſtood by all parties; and principally becauſe, by uſing the words of the old theory, I am at liberty to define them, and to give ſignifications expreſſive merely of facts, and of the actual ſtate of bodies; whereas the language and theory of the antiphlogiſtic chemiſts being interwoven and adapted to each other, the former cannot be diveſted of its theoretical reference, and therefore ſeems inapplicable to the mere expoſition of facts, but ought to be reſerved ſolely for the explanation of the doctrines from which this language is derived. Thus by the definition which I have mentioned of *phlogiſtication*, this word expreſſes not the preſence or exiſtence of an hypothetical principle of inflammability; but a certain well-known quality of acids and of other bodies, communicated to them by the addition of many actual inflammable ſubſtances. Thus nitrous acid acquires a phlo-

giſticated quality by addition of a little ſpirit of wine, or by diſtillation with any inflammable ſubſtance.

3. No two ſubſtances are more frequently in the hands of chemiſts and artiſts than *vitriolic acid* and *nitre*, yet I have found, that a mere mixture of theſe, when much concentrated, poſſeſſes properties which neither the vitriolic acid nor the nitrous, of the ſame degree of concentration, have ſingly, and which could not eaſily be deduced, *à priori*, by reaſoning from our preſent knowledge of the theory of chemiſtry.

4. Having found by ſome previous trials that a mixture compoſed of nitre diſſolved in oil of vitriol was capable of diſſolving ſilver eaſily and copiouſly, while it did not affect copper, iron, lead, regulus of cobalt, gold, and platina, I conceived, that it might be uſeful in ſome caſes of the parting of ſilver from copper and the other metals above mentioned; and having alſo obſerved, that the diſſolving powers of the mixture of vitriolic and nitrous acids varied greatly in different degrees of concentration and phlogiſtication, I thought that an inveſtigation of theſe effects might be a ſubject fit for philoſophical chemiſtry, and might tend to illuſtrate the theory of the diſſolution of metals in acids. With theſe views I made the following experiments.

5. I put into a long-necked retort, the contents of which, including the neck, were 1400 grain meaſures, 100 grain meaſures of oil of vitriol of the uſual denſity at which it is prepared in England, that is, whoſe ſpecific gravity is to that of water as 1,844 to 1, and 100 grains of pure and clean nitre, which was then diſſolved in the acid by the heat of a water-bath. To this mixture 100 grains of ſtandard ſilver were added; the retort was ſet in a water bath, in which the

water

water was made to boil, and a pneumatic apparatus was applied to catch any air or gas which might be extricated.

The ſilver began to diſſolve, and the ſolution became of a purple or violet colour. No air was thrown into the inverted jar, excepting a little of the common air of the retort, by means of the expanſion which it ſuffered from the heat of the water-bath, and from ſome nitrous fumes which appeared in the retort, and which having afterwards condenſed, occaſioned the water to riſe along the neck of the retort, and mix with the ſolution. The remaining ſilver was then ſeparated and weighed, and it was found that 39 grains had been diſſolved; but probably more would have been diſſolved if the operation had not been interrupted by the water ruſhing into the retort.

6. In the ſame apparatus 200 grains of ſtandard ſilver were added to a mixture of 100 grains of nitre previouſly diſſolved in 200 grain-meaſures of oil of vitriol; and in this ſolvent 92 grains of the ſilver were diſſolved, without any production of air or gas. The ſolution, which was of a violet colour, having been poured out of the retort while warm (for with ſo large a proportion of nitre, ſuch mixtures, eſpecially after having diſſolved ſilver, are apt to congeal with ſmall degrees of cold), in order to ſeparate the undiſſolved ſilver from it, and having been returned into the retort without this ſilver, I poured 200 grains of water into the retort, upon which a ſtrong effervеſcence took place between the ſolution and the water, and 3100 grain-meaſures of nitrous gas were thrown into the inverted jar. Upon pouring 200 grains more of water into the retort, 600 grain-meaſures of the ſame gas were expelled. Further additions of water yielded no more gas; neither did the ſilver, when afterwards added to this diluted ſolution, give

any ſenſible efferveſcence, or ſuffer a greater loſs of weight than two grains.

7. In the ſame apparatus 100 grains of ſtandard ſilver were expoſed to a mixture of 30 grains of nitre diſſolved in 200 grain-meaſures of oil of vitriol; and in this operation, 80 grains of ſilver were diſſolved, while at the ſame time 4500 grain-meaſures of nitrous gas were thrown into the inverted jar. When the undiſſolved ſilver was removed, 200 grains of water were added to the ſolution, which was of a violet colour, and upon the mixture of the two fluids an efferveſcence happened; but only a few bubbles of nitrous gas were then expelled.

8. In the ſame apparatus 100 grains of ſtandard ſilver were expoſed to a mixture of 200 grain-meaſures of oil of vitriol, 200 grains of nitre, and 200 grains of water; and in this operation 20 grains of the ſilver were diſſolved without any ſenſible emiſſion of air or gas.

9. In theſe experiments, the copper contained in the ſtandard ſilver gave a reddiſh colour to the ſaline maſs which was formed in the ſolution, and ſeemed to be a calx of copper interſperſed through the ſalt of ſilver. I perceived no other difference between the effects of pure and ſtandard ſilver diſſolved in this acid.

10. I then expoſed *tin* to the ſame mixture of oil of vitriol and nitre, in the ſame apparatus, and in the ſame circumſtances, taking care always to add more metal than could be diſſolved, that, by weighing the remainder, the quantity capable of being diſſolved might be found, as I had done with the experiments on ſilver: and the reſults were as follow.

11. No tin was diſſolved nor calcined by the mixtures in the proportion of 200 grain-meaſures of oil of vitriol to 200 grains

of

of nitre; nor by another mixture in the proportion of 200 grain-meaſures of oil of vitriol to 150 grains of nitre, and conſequently no gas was produced in either inſtance.

12. With a mixture in the proportion of 200 grain-meaſures of oil of vitriol and 100 grains of nitre, the tin began ſoon to be acted on, and to be diffuſed through the liquor; but no extrication of gas appeared until the digeſtion had been continued two hours in boiling water; and then it took place, and gave a frothy appearance to the mixture, which was of an opaque white colour, from the powder of tin diffuſed among it. In this experiment the quantity of tin thus calcined was 73 grains, and the quantity of nitrous gas extricated during this action on the tin was 8500 grain-meaſures. Then, upon pouring 200 grains of water into the retort, a freſh effervescence took place between the water and the white opaque maſs, and 4600 grain-meaſures of nitrous gas were thrown into the inverted receiver.

13. With a mixture in the proportion of 100 grain-meaſures of oil of vitriol to 30 grains of nitre, 30 grains of tin were diſſolved or calcined, and the nitrous gas, which began to be extricated much ſooner than in the laſt mentioned experiment with a larger proportion of nitre, amounted to 6300 grain-meaſures. Water, added to this ſolution of tin, did not produce any effervescence.

14. With a mixture in the proportion of 200 grain-meaſures of oil of vitriol, 200 grains of nitre, and 200 grains of water, 133 grains of tin were acted on with an effervescence, which took place violently, and produced 6500 grain-meaſures of nitrous gas.

15. The ſeveral mixtures above mentioned, in different proportions of nitre and oil of vitriol, did, by the help of the heat

heat of the water-bath, calcine *mercury* into a white or greyiſh powder. *Nickel* was alſo partly calcined and partly diſſolved by theſe mixtures. I did not perceive that any other metal was affected by them, excepting that the ſurfaces of ſome of them were tarniſhed.

16. Theſe mixtures of oil of vitriol and nitre were apt to congeal by cold, thoſe eſpecially which had a large proportion of nitre. Thus, a mixture of 1000 grain-meaſures of oil of vitriol and 480 grains of nitre, after having kept fluid ſeveral days, in a phial not ſo accurately ſtopped as to prevent altogether the eſcape of ſome white fumes, congealed at the temperature of 55° of FAHRENHEIT's thermometer; whereas ſome of the ſame liquid, having been mixed with equal parts of oil of vitriol, did not congeal with a leſs cold than 45°. The congelation is promoted by expoſure to air, by which white fumes riſe, and moiſture may be abſorbed, or by any other mode of ſlight dilution with water.

17. Dilution of this compound acid, with more or leſs water, alters conſiderably its properties, with regard to its action on metals. Thus it has been obſerved, that in its concentrated ſtate it does not act on *iron*; but, by adding water, it acquires a power of acting on that metal, and with different effect, according to the proportion of the water added. Thus, by adding to two meaſures of the compound acid one meaſure of water, the liquor is rendered capable of calcining iron, and forming with it a white powder, but without effervеſcence. With an equal meaſure of water effervеſcence was produced. With a larger proportion of water the iron gave alſo a brown colour to the liquor, ſuch as phlogiſticated nitrous acid acquires from iron, or communicates to a ſolution of martial vitriol in water.

18. Dilution

18. Dilution with water renders this compound acid capable of diſſolving *copper* and *zinc*, and probably thoſe other metals which are ſubject to the action of the dilute vitriolic or nitrous acids.

SECTION SECOND.

An account of a new proceſs for ſeparating ſilver from copper.

19. The properties of this liquor, in diſſolving ſilver eaſily without acting on copper, have rendered it capable of a very uſeful application in the arts. Among the manufactures at Birmingham, that of making veſſels of ſilver plated on copper is a very conſiderable one. In cutting out the rolled plated metal into pieces of the required forms and ſizes, there are many ſhreds, or *ſcraps* as they are called, unfit for any purpoſe but the recovery of the metals by ſeparating them from each other. The eaſieſt and moſt œconomical method of parting theſe two metals, ſo as not to loſe either of them, is an object of ſome conſequence to the manufacturers. For this purpoſe two modes were practiſed; one, by melting the whole of the mixed metals with lead, and ſeparating them by eliquation and teſting; and the ſecond, by diſſolving both metals in oil of vitriol, with the help of heat, and by ſeparating the vitriol of copper, by diſſolving it in water, from the vitriol of ſilver, which is afterwards to be reduced and purified. In the firſt of theſe methods, there is a conſiderable waſte of lead and copper; and in the ſecond, the quantity of vitriolic acid employed is very great, as much more is diſſipated in the form of volatile vitriolic, or ſulphureous acid, than remains in the compoſition of the two vitriols.

Some years ago, I communicated to an artiſt the method of effecting the ſeparation of ſilver and copper by means of the above-mentioned compound of vitriolic acid and nitre; and, as I am informed, that it is now commonly practiſed by the manufacturers in Birmingham, I have no doubt but it is much more œconomical, and it is certainly much more eaſily executed than any of the other methods: for nothing more is required than to put the pieces of plated metal into an earthen glazed pan; to pour upon them ſome of the acid liquor, which may be in the proportion of eight or ten pounds of oil of vitriol to one pound of nitre; to ſtir them about, that the ſurfaces may be frequently expoſed to freſh liquor, and to aſſiſt the action by a gentle heat from 100° to 200° of FAHRENHEIT's ſcale. When the liquor is nearly ſaturated, the ſilver is to be precipitated from it by common ſalt, which forms a luna cornea, eaſily reducible by melting it in a crucible with a ſufficient quantity of pot-aſh; and, laſtly, by refining the melted ſilver, if neceſſary, with a little nitre thrown upon it. In this manner the ſilver will be obtained ſufficiently pure, and the copper will remain unchanged. Otherwiſe, the ſilver may be precipitated in its metallic ſtate, by adding to the ſolution of ſilver a few of the pieces of copper, and a ſufficient quantity of water to enable the liquor to act upon the copper.

The property which this acid mixture poſſeſſes of diſſolving ſilver with great facility, and in conſiderable quantity, will probably render it an uſeful menſtruum in the ſeparation of ſilver from other metals; and as the alchemiſts have diſtinguiſhed the peculiar ſolvent of gold under the title of *aqua regis*, a name ſufficiently diſtinctive, though founded on a fanciful alluſion; ſo, if they had been acquainted with the

properties

properties of this compound, they would probably have beſtowed on it the appellation of *aqua reginæ*.

SECTION THIRD.

The change of properties communicated to the mixture of vitriolic and nitrous acids by phlogiſtication.

20. The above-deſcribed compound acid may be phlogiſticated in different methods, of which I ſhall mention three.

1ſt, By digeſting the compound acid with ſulphur by means of the heat of a water-bath, the liquor diſſolves the ſulphur with effervescence, loſes its property of yielding white fumes; and if the quantity of ſulphur be ſufficient, and if the heat applied be long enough continued, it exhibits red nitrous vapours, and aſſumes a violet colour.

2dly, If, inſtead of diſſolving nitre in concentrated vitriolic acid, this acid be impregnated with nitrous gas, or with nitrous vapour, by making this gas or vapour paſs into the acid, this compound will be phlogiſticated, as it contains not the entire nitrous acid, but only its phlogiſticated part, or element, the nitrous gas, without the proportion of pure air neceſſary to conſtitute an acid. This impregnation of oil of vitriol with nitrous gas or nitrous vapour was firſt deſcribed, and ſome of the properties of the impregnated liquor noticed, by Dr. PRIESTLEY. See Experiments and Obſervations on Air, Vol. III. p. 129 and 217.

3dly, By ſubſtituting nitrous ammoniac inſtead of nitre in the mixture with oil of vitriol.

21. The compound prepared by any of theſe methods, but eſpecially by the firſt and ſecond, differs conſiderably in its

properties with regard to its action on metals from the acid deſcribed in the firſt ſection. It has been obſerved, that the latter compound has little action on any metals but ſilver, tin, mercury, and nickel. On the other hand, the phlogiſticated compound not only acts on theſe, but alſo on ſeveral others. It forms with *iron* a beautiful roſe-coloured ſolution, without application of any artificial heat; and in time a roſe-coloured ſaline precipitate is depoſited, which is ſoluble in water with conſiderable efferveſcence. It diſſolves *copper*, and acquires from this metal, and alſo from regulus of *cobalt*, *zinc*, and *lead*, pretty deep violet tinges. *Biſmuth* and *regulus of antimony* were alſo attacked by this phlogiſticated acid.

To aſcertain more exactly the effects of this phlogiſticated acid on ſome metals, I made the following experiments, with a liquor prepared by making nitrous gas paſs through oil of vitriol during a conſiderable time.

22. To 200 grain-meaſures of the oil of vitriol impregnated with nitrous gas, put into a retort with a long neck, the capacity of which, including the neck, was 1150 grain-meaſures, I added 144 grains of ſtandard ſilver, and immerſed the mouth of the retort in water, under an inverted jar filled with water, to catch the gas which might be extricated.

The acid began to diſſolve the ſilver with efferveſcence by application of heat; the ſolution became of a violet colour, and the quantity of nitrous gas received in the inverted jar was 14700 grain-meaſures. Upon weighing the ſilver remaining, the quantity which had been diſſolved was found to be 70 grains. When water was added to the ſolution, an efferveſcence appeared, but only a very ſmall quantity of gas was extricated. By means of the water, a white ſaline powder of ſilver, ſoluble in a larger quantity of water, was precipitated from

from the ſolution. The ſolution of ſilver, when ſaturated and undiluted, congeals readily in cool temperatures, and, when diluted to a certain degree with water, gives foliated cryſtals.

23. In the ſame apparatus, and in the ſame manner, 100 grain-meaſures of this impregnated oil of vitriol were applied to *iron*. An efferveſcence appeared without application of heat, the ſurface of the iron acquired a beautiful roſe colour or redneſs mixed with purple: and this colour gradually pervaded the whole liquor, but diſappeared on keeping the retort ſome time in hot water. Notwithſtanding a conſiderable apparent efferveſcence, the quantity of air expelled into the inverted jar was only 400 grain-meaſures, of which $\frac{1}{4}$ was nitrous, and the reſt phlogiſticated. The ſolution was then poured out of the retort, and the iron was found to have loſt only two grains in weight. The ſolution was returned into the retort, without the iron, and 200 grains of water were added to it; upon which a white powder was immediately precipitated, which re-diſſolved with great efferveſcence. When 2000 grain-meaſures of nitrous gas had been expelled into the inverted jar, without application of heat, the retort was placed in the water-bath, the heat of which rendered the efferveſcence ſo ſtrong, that the liquor boiled over the neck of the retort, ſo that the quantity of gas extricated could not be aſcertained.

24. In the ſame manner 11 grains of *copper* were diſſolved in 100 grain-meaſures of impregnated oil of vitriol. The ſolution was of a deep violet colour, and at laſt was turbid. The quantity of nitrous gas expelled into the inverted jar during the operation was 4700 grain-meaſures. When the copper was removed, and 200 grains of water were added to the ſolution, an efferveſcence took place, 1700 grain-meaſures of

nitrous gas were expelled, and the ſolution then acquired a blue colour.

25. In the ſame apparatus and manner, 100 grain-meaſures of the impregnated oil of vitriol were applied to *tin*, which was thereby diminiſhed in weight 16 grains, while the liquor acquired a violet colour, became turbid by the ſuſpenſion of the calx of tin, and a quantity of nitrous gas was thrown into the inverted receiver equal to 4100 grain-meaſures, without application of heat, and another quantity equal to 4900 grain-meaſures, after the retort was put into the water-bath.

26. *Mercury* added to the impregnated oil of vitriol formed a thick white turbid liquor, which was rendered clear by addition of unimpregnated oil of vitriol. In a little time this mixture continuing to act on the remaining mercury acquired a purple colour. The mercury acted upon ſunk to the bottom of the glaſs in the form of a white powder, and the purple liquor, when mixed with a ſolution of common ſalt in water, gave no appearance of its containing any mercury in a diſſolved ſtate.

27. The nitrous gas with which the oil of vitriol is impregnated ſhews no diſpoſition to quit the acid by expoſure to air; but, on adding water to the impregnated acid, the gas is expelled ſuddenly with great effervescence, and with red fumes, in conſequence of its mixture with the atmoſpherical air.

Upon adding 240 grains of water to 60 grain-meaſures of impregnated oil of vitriol, 2300 grains of nitrous gas were thrown into the receiver; but as the action of the two liquors is inſtantaneous, the quantity of gas expelled from the retort before its neck could be immerſed in water, and placed under the receiver, muſt have been conſiderable. The whole of the gas, however, was not extricated by means of the water, for

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the remaining liquor diſſolved 5 grains of copper, while 800 meaſures of nitrous gas were thrown into the retort.

28. The following facts principally are eſtabliſhed by the preceding experiments.

1. That a mixture of the vitriolic and nitrous acids in a concentrated ſtate has a peculiar faculty of diſſolving ſilver copiouſly.

2. That it acts upon, and principally calcines, tin, mercury, and nickel; the latter of which, however, it diſſolves in ſmall quantity, and that it has little or no action on other metals.

3. That the quantity of gas produced while the metal is diſſolving is greater, relatively to the quantity of metal diſſolved, when the proportion of nitre to the vitriolic acid is ſmall than when it is large; and that when the metals are diſſolved by mixtures containing much nitre, and with a ſmall production of gas, the ſolution itſelf, or the metallic ſalt formed in it, yields abundance of gas when mixed with water.

4. That dilution with water renders the concentrated mixture leſs capable of diſſolving ſilver, but more capable of acting on other metals.

5. That this mixture of highly concentrated vitriolic and nitrous acids acquires a purple or violet colour when phlogiſticated, either by addition of inflammable ſubſtances as ſulphur, or by its action on metals, or by very ſtrong impregnation of oil of vitriol with nitrous gas *.

* Dr. PRIESTLEY has noticed this colour communicated to oil of vitriol by impregnation with nitrous gas or vapour, and alſo the effervescence produced by adding water to this impregnated liquor. See Experiments and Obſervations, Vol. III. p. 129 and 217.

6. That

6. That this phlogiſtication was found to communicate to the mixture the power of diſſolving, though in ſmall quantities, copper, iron, zinc, and regulus of cobalt.

7. That water expels from a highly phlogiſticated mixture of concentrated vitriolic and nitrous acids, or of oil of vitriol impregnated with nitrous gas, a great part of its contained gas; and that therefore this gas is not capable of being retained in ſuch quantity by dilute as by concentrated acids. Water unites with the mixture of oil of vitriol and nitre, without any conſiderable effervеſcence.

29. To theſe obſervations I ſhall ſubjoin one other fact, namely, that, when to the mixture of oil of vitriol with nitre a ſaturated ſolution of common ſalt in water is added, a powerful aqua regis is produced, capable of diſſolving gold and platina; and this aqua regis, though compoſed of liquors perfectly colourleſs and free from all metallic matter, acquires at once a bright and deep yellow colour. The addition of dry common ſalt to the concentrated mixtures of vitriolic and nitrous acids produces an effervеſcence, but not the yellow colour; for the production of which therefore a certain proportion of water ſeems to be neceſſary.

PART II.

On the precipitation of ſilver from nitrous acid by iron.

SECTION I.

1. BERGMAN relates, that upon adding iron to a ſolution of ſilver in the nitrous acid no precipitation enſued*; although

* Diſſert. de Phlog. quantitate in Metallis.

the affinity of iron to acids in general is known to be much ſtronger than that of ſilver; and although, even with regard to the nitrous acid, other experiments evince the ſuperior affinity of iron: for as iron precipitates copper from this acid, and as copper precipitates ſilver, we muſt infer the greater affinity of iron than of ſilver. In the courſe of his experiments, however, ſome inſtances of precipitation occurred, which he attributed to the peculiar quality of the irons which he then employed *. I was deſirous of diſcovering the circumſtances,

* BERGMAN tried many different kinds of iron, and he thought he found two which were capable of precipitating ſilver. But as he did not diſcover the circumſtances according to which this precipitation ſometimes does, and at other times does not happen, he may have been miſtaken with regard to the peculiar quality of theſe two kinds of iron. At leaſt the ſeveral kinds which I have tried always precipitated ſilver in certain circumſtances, and always failed to precipitate in certain other circumſtances. I do not know any other author who has mentioned this ſubject, excepting Mr. KIRWAN; who, in the concluſion of his valuable Papers on *the Attractive Powers of Mineral Acids*, ſays, "I have always "found ſilver to be eaſily precipitated from its ſolution in the nitrous acid by iron. "The ſum of the quieſcent affinities being 625, and that of the divellent 746. "Yet Mr. BERGMAN obſerved, that a very ſaturated ſolution of ſilver was very "difficultly precipitated, and only by ſome ſorts of iron, even though the ſolution "was diluted, and an exceſs of acid added to it. The reaſon of this curious "phænomenon appears to me deducible from a circumſtance firſt obſerved by "SCHEELE, in diſſolving mercury, namely, that the nitrous acid when ſaturated "with it will take up more of it in its metallic form. The ſame thing happens "in diſſolving ſilver in the nitrous acid in a ſtrong heat; for, as I before "remarked, the laſt portions of ſilver thrown in afford no air, and conſequently "are not dephlogiſticated. Now this compound of calx of ſilver, and ſilver in "its metallic form, may well be unprecipitable by iron, the ſilver in its metallic "form preventing the calx from coming into contact with the iron, and extracting "phlogiſton from it." In this Paper I ſhall not enter into the explanation of theſe appearances; but I thought it neceſſary to premiſe what ſo eminent a chemiſt as Mr. KIRWAN has ſuggeſted on the ſubject, that the reader may ſee at once the preſent ſtate of the queſtion. I ſhall only remark, that the above explanation,

ſtances, and of inveſtigating the cauſe, if I ſhould be able, of this irregularity and exception to the generally received laws of affinity.

2. I digeſted a piece of fine ſilver in pure and pale nitrous acid, and while the diſſolution was going on, and before the ſaturation was completed, I poured a portion of the ſolution upon pieces of clean and newly-ſcraped iron wire into a wine glaſs, and obſerved a ſudden and copious precipitation of ſilver. The precipitate was at firſt black, then it aſſumed the appearance of ſilver, and was five or ſix times larger in diameter than the piece of iron wire which it enveloped. The action of the acid on the iron continued ſome little time, and then it ceaſed; the ſilver re-diſſolved, the liquor became clear, and the iron remained bright and undiſturbed in the ſolution at the bottom of the wine glaſs, where it continued during ſeveral weeks, without ſuffering any change, or effecting any precipitation of the ſilver.

3. When the ſolution of ſilver was completely ſaturated, it was no longer affected by iron, according to BERGMAN's obſervation.

4. Having found that the ſolution acted on the iron, and was thereby precipitated, before it had been ſaturated, and not afterwards, I was deſirous of knowing, whether the *ſaturation* was the circumſtance which prevented the action and precipitation. For this purpoſe, I added to a portion of the ſaturated ſolution ſome of the ſame nitrous acid, of which a part had been employed to diſſolve the ſilver; and into this mixture,

tion, not being founded on any peculiarity in the nature of iron, ſeems to ſuppoſe, that the ſilver is alſo incapable of being precipitated, from ſuch ſolutions as iron cannot act upon, by any other metal. But this is not the caſe: copper and zinc readily precipitate ſilver from theſe ſolutions.

abounding

abounding with a ſuperfluous acid, I threw a piece of iron, but no precipitation occurred. It was thence evident, that the ſaturation of the acid was not the only circumſtance which prevented the precipitation.

5. To another portion of the ſaturated ſolution of ſilver I added ſome red ſmoking nitrous acid; and I found, upon trial, that iron precipitated the ſilver from this mixture, and that the ſame appearances were exhibited as had been obſerved with the ſolution previouſly to its ſaturation.

6. The ſame effects were produced when vitriolic acid was added to the ſaturated ſolution of ſilver, and iron afterwards applied.

7. To ſome of the ſame nitrous acid, of which a part had been employed to diſſolve the ſilver, I added a piece of iron; and, while the iron was diſſolving, I poured into the liquor ſome of the ſaturated ſolution of ſilver; upon which a precipitation of ſilver took place inſtantly; although, when the ſame acid had been previouſly mixed with the ſolution of ſilver, and the iron was then added to the mixture, no precipitation had enſued.

8. The quantity of vitriolic acid, or of the red fuming nitrous acid, neceſſary to communicate to the ſaturated ſolution of ſilver the property of being acted on by iron, varies according to the concentration, and to the degree of phlogiſtication of the acids added; ſo that a leſs quantity than is ſufficient does not produce any apparent effect. Nevertheleſs, when the ſolution of ſilver is by addition of theſe acids brought nearly to a precipitable ſtate, the addition of ſpirit of wine will, in a little time, render it capable of acting on iron.

9. It appears then, that a ſolution of ſilver is not precipitated by iron in cold, unleſs it have a ſuperabundance of phlogiſticated acid *.

10. Heat affects the action of a ſolution of ſilver on iron: for if iron be digeſted with heat, in a perfectly ſaturated ſolution of ſilver, ſuch as a ſolution of cryſtals of nitre of ſilver in water, the ſilver will be depoſited in its bright metallic ſtate on different parts of the iron, and the iron which has been acted on by the ſolution appears in form of a yellow ochre.

11. BERGMAN relates, that he has ſometimes obſerved beautiful cryſtallizations or vegetations of metallic ſilver formed on pieces of iron immerſed long in a ſolution of ſilver.

I have found that no time is able to effect this depoſition, unleſs the ſolution be in a ſtate *nearly* ſufficiently phlogiſticated to admit of a precipitation by iron, but not completely phlogiſticated enough to effect that purpoſe immediately.

12. Dilution with a great deal of water ſeemed to diſpoſe the ſolutions of ſilver to be precipitated by iron more eaſily.

* It was ſaid, at § 4. that the addition of dephlogiſticated nitrous acid to a ſaturated ſolution of ſilver did not render this ſolution precipitable by iron. Nevertheleſs, as this acid diſſolves iron, ſuch a quantity may be added, as to overcome the counteracting quality of the ſolution of ſilver, ſo that the acid ſhall be able to act on the iron; and while this metal is diſſolving, it phlogiſticates the mixture, which then becomes capable of being precipitated, and is in fact reduced to the ſame circumſtances as are deſcribed at § 7. The limits of the quantities which produce changes cannot be aſcertained, becauſe they depend on the degrees of concentration and phlogiſtication of the ſubſtances employed; and therefore, whenever a change is ſaid to be produced by a certain ſubſtance, it means that it may be produced by *ſome* proportion, but does not imply by *every* proportion, of that ſubſtance. Without attending to theſe conſiderations, perſons trying to repeat the experiments mentioned in this Paper will be liable to be deceived.

A ſolution

A ſolution of ſilver, which did not act on iron, upon being very much diluted, and having a piece of iron immerſed in it, during ſeveral hours, gave a precipitate of ſilver in the form of a black powder.

SECTION II.

On the alterations which iron or its ſurface undergoes by the action of a ſolution of ſilver in nitrous acid, or of a pure concentrated nitrous acid.

13. It has been ſaid, that when iron is expoſed to the action of a phlogiſticated ſolution of ſilver, it inſtantly precipitates the ſilver, is itſelf acted upon or diſſolved by the acid ſolution during a certain time, longer or ſhorter, according to the degree of phlogiſtication, quantity of ſuperabundant acid, and other circumſtances, and that at length the ſolution of the iron ceaſes; the ſilver precipitate is re-diſſoved, if there is ſuperfluous acid; the liquor becomes clear again, but only rendered a little browner by its having diſſolved ſome iron; while the piece of iron remains bright and undiſturbed at the bottom of the liquor, where it is no longer able to affect the ſolution of ſilver.

14. I poured a part of the phlogiſticated ſolution of ſilver which had paſſed through theſe changes, and which had ceaſed to act upon the piece of iron, into another glaſs, and dropped another piece of freſh iron wire into the liquor; upon which I obſerved a precipitation of ſilver, a ſolution of part of the iron, a rediſſolution of the precipitated ſilver, and a ceſſation of all theſe phænomena, with the iron remaining bright and quiet at the bottom of the liquor, as before. It appeared

then, that the liquor had not loſt its power of acting on *freſh* iron, although it ceaſed to act on that piece which had been expoſed to it.

15. To one of the pieces of iron which had been employed in the precipitation of a ſolution of ſilver, and from which the ſolution, no longer capable of acting upon it, had been poured off, I added ſome phlogiſticated ſolution of ſilver which had never been expoſed to the action of iron, but no precipitation happened. It appeared then, that the iron itſelf, by having been once employed to precipitate a ſolution of ſilver, was rendered incapable of any further action on any ſolution of ſilver. And it is to be obſerved, that this alteration was produced without the leaſt diminution of its metallic ſplendour, or change of colour. The alteration, however, was only ſuperficial, as may be ſuppoſed; for by ſcraping off its altered coat, it was again rendered capable of acting on a ſolution of ſilver. To avoid circumlocution, I ſhall call iron thus affected, *altered* iron; and iron which is clean, and has not been altered, *freſh* iron.

16. To a phlogiſticated ſolution of ſilver, in which a piece of bright altered iron lay, without action, I added a piece of freſh iron, which was inſtantly enveloped with a maſs of precipitated ſilver, and acted on as uſual; but what is very remarkable, in about a quarter of a minute, or leſs, the altered iron ſuddenly was covered with another coat of precipitated ſilver, and was now acted on by the acid ſolution like the freſh piece. In a little time the ſilver precipitate was re-diſſolved, as uſual, and the two pieces of iron were reduced to an altered ſtate. When a freſh piece of iron was then held in the liquor, ſo as not to touch the two pieces of altered iron, they were nevertheleſs ſoon acted upon by the acid ſolution,

and ſuddenly covered with ſilver precipitate as before; and theſe phænomena may be repeated with the ſame ſolution of ſilver, until the ſuperfluous acid of the ſolution becomes ſaturated by the iron, and then the re-diſſolution of the precipitated ſilver muſt ceaſe.

17. I poured ſome dephlogiſticated nitrous acid on a piece of altered iron, without any action enſuing, although this acid readily acted on freſh iron, and when to the dephlogiſticated nitrous acid, with a piece of altered iron lying immerſed in it, I added a piece of freſh iron, this immediately began to diſſolve, and ſoon afterwards the altered iron was acted on alſo by the acid.

18. I poured upon a piece of altered iron a ſolution of copper in nitrous acid; but the copper was not precipitated by the iron; neither did this iron precipitate copper from a ſolution of blue vitriol.

19. Altered iron was acted on by a dilute phlogiſticated nitrous acid; but not by a red concentrated acid, which is known to be highly phlogiſticated.

20. I put ſome pieces of clean freſh iron wire into a concentrated and red fuming nitrous acid. No apparent action enſued; but the iron was found to be altered in the ſame manner as it is by a ſolution of ſilver; that is, it was rendered incapable of being attacked either by a phlogiſticated ſolution of ſilver, or by dephlogiſticated nitrous acid.

21. Iron was alſo altered by being immerſed ſome little time in a ſaturated ſolution of ſilver, which did not ſhew any viſible action on it.

22. The alteration thus produced on the iron is very ſuperficial. The leaſt rubbing expoſes ſome of the freſh iron beneath

neath the ſurface, and thus ſubjects it to the action of the acid.

It is therefore with difficulty that theſe pieces of altered iron can be dried, without loſing their peculiar property. For this reaſon, I generally transferred them out of the ſolution of ſilver or concentrated nitrous acid into any other liquor, the effects of which I wanted to examine. Or they may be transferred firſt into a glaſs of water, and thence into the liquor to be examined. But it is to be obſerved, that if they are allowed to remain long in the water, they loſe their peculiar property or alteration. They may be preſerved in their altered ſtate by being kept in ſpirit of ſal ammoniac.

23. To a ſaturated ſolution of copper in nitrous acid (which was capable of being readily precipitated by freſh iron) I added ſome ſaturated ſolution of ſilver. From this mixture a piece of freſh iron neither precipitated ſilver nor copper: nor did the addition of ſome dephlogiſticated nitrous acid effect this precipitation.

24. A ſolution of copper, formed by precipitating ſilver from nitrous acid by means of copper, was very reluctantly and ſlowly precipitated by a piece of freſh iron; and the iron thus acted on by the acid was changed to an ochre.

25. A ſaturated ſolution of ſilver having been *partly* precipitated by copper, acquired the property of acting upon freſh iron, and of being thereby precipitated.

26. Freſh iron immerſed ſome time in ſolutions of nitre of lead, or of nitre of mercury in water, did not occaſion any precipitation of the diſſolved metals; but acquired an *altered* quality. Theſe metals then in this reſpect reſemble ſilver.

27. It is well known, that a ſolution of martial vitriol, added to a ſolution of gold in aqua regis, precipitates the gold

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in its metallic ſtate. I do not recollect, that the precipitation of a ſolution of ſilver by the ſame martial vitriol has been obſerved. However, upon pouring a ſolution of martial vitriol into a ſolution of ſilver in the nitrous acid, a precipitate will be thrown down, which acquires in a few minutes more and more of a metallic appearance, and is indeed perfect ſilver. When the two ſolutions are pretty concentrated, a bright argentine film ſwims on the ſurface of the mixture, or ſilvers the ſides of the glaſs in which the experiment is made. When a phlogiſticated ſolution of ſilver is uſed, the mixture is blackened, as happens generally to a ſolution of martial vitriol, when a phlogiſticated nitrous acid is added to it.

I added about equal parts of water to a mixture of a phlogiſticated ſolution of ſilver and a ſolution of martial vitriol, in which *all* the ſilver had been precipitated, and digeſted the diluted mixture with heat, by which means moſt of the precipitated ſilver was re-diſſolved. BERGMAN has obſerved a ſimilar re-diſſolution of gold precipitated by martial vitriol upon boiling the mixture; but he attributes the re-diſſolution to the concentration of the aqua regis by the evaporation. As this explanation did not accord with my notions, I diluted the mixture with water, and found that the ſame re-diſſolution occurred both with the ſolution of ſilver and with that of gold. But with neither of the metals did I find that the re-diſſolution ever took place, unleſs there had been a ſuperabundant acid in the ſolutions of gold and ſilver employed.

28. Mercury is alſo precipitated in its metallic ſtate from its ſolution in nitrous acid by a ſolution of martial vitriol. When the liquor is poured off from the precipitate, this may be changed into running mercury by being dried near the fire.

29. I

29. I found alſo, that ſilver may be precipitated in its metallic ſtate, from its ſolution in vitriolic acid, by addition of a ſolution of martial vitriol. A vitriol of mercury may alſo be decompounded by a ſolution of martial vitriol, and the mercurial precipitate, which is a black powder, forms globules, when dried and warmed.

30. Luna cornea is not decompounded by martial vitriol; conſequently there is no operation of a double affinity. Nevertheleſs, this luna cornea may be decompounded by the elements of martial vitriol, while they are in the act of diſſolution; that is, the ſilver may be precipitated in its metallic ſtate, by digeſting luna cornea with a dilute vitriolic acid, to which ſome pieces of iron are added. And it is to be obſerved, that this reduction of the ſilver and precipitation take place, while the acid is yet unſaturated. Marine acid and iron applied to luna cornea effect the ſame reduction of the ſilver to a metallic ſtate, even when there is more acid than is ſufficient for both metals.

The explanation of theſe phænomena will be attempted in the ſubſequent Papers which I propoſe to preſent on this ſubject to the Society.

XXI. *Determination of the Longitudes and Latitudes of ſome remarkable Places near the Severn. In a Letter from* Edward Pigott, *Eſq. to Sir* Henry C. Englefield, *Bart. F. R. S.*

Read May 20, 1790.

AS perhaps this Paper may be thought of ſome uſe, I beg you will do me the favour of preſenting it to the Royal Society. Its principal object is to ſettle the longitude and latitude of ſeveral remarkable places near the Severn, the relative diſtances of which I meaſured trigonometrically during my ſtay in Glamorganſhire. As they are all deduced from Frampton-houſe, it is requiſite to determine the poſition of that place with correctneſs. My father has already given its longitude in the Philoſophical Tranſactions, Vol. LXXI. being the mean of ſeveral obſervations of Jupiter's firſt and ſecond ſatellites. From the known ability of the obſerver, we may undoubtedly depend that all poſſible exactneſs was obtained, of which thoſe obſervations are ſuſceptible; but at that time the ſuperior accuracy of the lunar tranſits was not known; therefore at preſent there can be no heſitation in giving the preference to the following reſults.

Difference of longitudes in time between Greenwich and Frampton-houſe, deduced from obſerved meridian tranſits of the moon's limbs.

Dates.	Difference of meridians, weſt.	Stars obſerved with ☽. G. Greenwich. F. Frampton.
1778		
Oct. 3.	14′ 28″ –	β Aquilæ at G. and F..
15.	14 36	γ Aquilæ at G. and F.
Nov. 25.	14 25 +	☽ obſerved only at 3 wires at F. υ ♒ and MAYER's N° 891. at F. Fomalhaut at G.
Dec. 26.	14 28	δ ♒ η ♓ at F. α ♈ ≃ ♉ at G.
1779		
Feb. 22.	14 26 –	ι ♉ ζ ♉ at F. α ♈ α Ceti at G.
27.	14 31	γ ♋ at G. and F.
Mar. 24.	14 37	γ Canis maj. ♌ at F. α ♉ α ♌ at G.
30.	14 41	b ♍ at F. α ♉ α ♌ at G.
31.	14 37	ι ♍ at F. α ♉ at G.
31.	14 43 –	Ditto. ditto.
Apr. 24.	14 27 +	☽ obſerved only at two wires at F; but they agree. β ♍ at G. and at F.
May 22.	14 26½	α ♌ at G. and at F.
Oct. 22.	14 33½	Fomalhaut at G. and at F.
Diff. of meridians	14 32 +	on a mean.

Theſe are all the obſervations I have reduced except three doubtful ones, which were computed merely out of curioſity.

1778	′ ″	
Nov. 1.	14 20—	☽ and Arcturus; 10+hours interval; going of clock uncertain; F. α ♈ at G.
30.	14 47	☽ obſerved only at two wires. ☽ and Arcturus; 12½ hours interval; going of clock uncertain.
1779		
Apr. 26.	14 14½	☽ obſerved only at three wires, and they do not agree; F.

This method of determining terreſtrial longitudes I have fully detailed in the Philoſophical Tranſactions, Vol. LXXVI. and ſtill think it cannot be too ſtrongly recommended. The preceding additional ſet of reſults do further corroborate the reliance that may be put on it, though the obſervations were not made with that intention, and conſequently ſeveral of them are deficient in many particulars: their agreement, nevertheleſs, is concluſive, and infinitely more ſatisfactory than could be expected. Since the above-mentioned publication I have been informed, that M. le Marquis DE CHABERT and others, many years ago, ſettled differences of meridians on ſimilar principles, and I dare ſay with as much ſagacity as the then imperfect ſtate of the method would permit. At preſent it is certainly conſiderably improved, being ſuſceptible of very great exactneſs and facility, which perhaps may be conſidered as the ſole requiſites for rendering it any wiſe uſeful.

The latitude of the ſame place, taken with an 18-inch quadrant made by BIRD, is thus by my obſervations:

51°	25′	0″	by β Geminorum.
51	24	50	by ε Bootis; two obſervations.
51	25	5	by π Sagittarii.
51	25	13	by γ Virginis.
51	24	55	by η Bootis; two obſervations.
51	24	48	by γ Delphini.
51	24	56	by Fomalhaut.
51	24	58 +	on a mean.
51	25	1	{ ditto, by my father, with the ſame inſtrument. See Phil. Tranſ. Vol. LXXI.
51	25	0 −	latitude of Frampton-houſe on a mean.

Having thus ſettled the poſition of the Obſervatory, I may now proceed to give the particulars of the trigonometrical operations.

I meaſured the ſame baſe three times by different methods, the reſults were $\left.\begin{matrix}2046\\2042\\2042\end{matrix}\right\}$ feet. As the view from its extremities was very confined, another baſe of 1861 yards was deduced from it, ſituated on the high lands that edge the Severn, having a very extenſive and beautiful proſpect. From the extremities of this ſecond baſe, all the angles were taken with a tolerably good theodolite on which two minutes might be eaſily read off. The reſults here given are the diſtances from the various places to the weſtern extremity of their baſe, their perpendicular diſtances to its meridian, and its diſtance from theſe perpendiculars.

Diſtances

Diſtances in yards,			
Direct.	To the meridian.	To the perpendicular.	
3307	1254 E	3059 N	Frampton-houſe.
45654	42239 E	17324 S	Brin Hill, the center.
36928	21853 E	29768 S	Quantock Hill, the eaſt part.
40446	15586 E	37322 S	Land Mark, a tower.
35543	11542 E	33617 S	Watchet Hill, the center.
21911	1465 E	21862 S	Minehead.
21336	6664 W	20268 S	Porlock, or Huſton Point.
30238	23152 W	19450 S	Leemouth.
46264	40398 W	22547 S	Hangman Hill.
2921	2842 W	673 N	St. Donat's Caſtle.
1564	491 E	1483 N	Llantwit Church.
10140	448 W	10130 N	Llangwynewar Hill, eaſt part.
25126	2299 E	25020 N	A remarkable hill.
3135	2063 E	2361 N	Llanmace Church.
8864	5906 E	6609 N	St. Hilary's Church.

The direct diſtances are the moſt accurate, the others being affected according to the exactneſs of the meridian of the weſt extremity of the baſe; the direction of which was found by the variation needle, its declination having been determined at Frampton-houſe, and therefore ſufficiently correct; for an error in that angle, even of half a degree, would make a difference of a very few ſeconds in any of the places obſerved.

The following are the longitudes and latitudes of the ſame places, deduced by Gen. Roy's moſt accurate and uſeful tables, ſhewing the value of each degree, &c.

Longitudes

Longitudes weſt of Greenwich, in time.	Longitudes weſt of Greenwich, in deg. &c.	Latitudes North.	
′ ″	° ′ ″	° ′ ″	
12 24 −	3 5 58	51 14 56½	Brin Hill, the center.
13 28 −	3 21 57	51 8 48½	Quantock Hill, eaſt part.
13 47½	3 26 52	51 5 5	Land-mark, a tower.
14 0 +	3 30 1	51 6 55	Watchet Hill, the center.
14 17½	3 34 23	51 26 44½	St. Hilary's Church.
14 29 −	3 37 12	51 35 49	A remarkable Hill.
14 29½	3 37 24	51 24 39	Llanmace Church.
14 31½	3 37 52	51 12 42½	Minehead.
14 32 +	3 38 2	51 25 0 −	Frampton-houſe.
14 34½	3 38 38	51 24 13	Llantwit Church.
14 36 +	3 39 1	51 23 29	Station, weſt extremity of the baſe.
14 37½	3 39 22	51 28 28½	Llangwynewar Hill, eaſt part.
14 45	3 41 15	51 23 49	St. Donat's Caſtle.
14 57 −	3 44 14	51 13 29½	Porlock or Huſton Point.
15 48½	3 57 7	51 13 54	Leemouth.
16 42½	4 10 35	51 12 22	Hangman Hill.

The names of the places here ſet down were pointed out to me by perſons who ſeemed well acquainted with the ſurrounding country. The breadth of the Severn at that part where I took the angles is about 12$\frac{1}{9}$ miles. However coarſe theſe trigonometrical operations may appear, when compared to thoſe made ſo ſcientifically, and with ſuch wonderful exactneſs, in the ſouthern parts of the kingdom, they nevertheleſs ſettle the geographical ſituations of the given places with more preciſion than is uſually obtained, even from a ſeries of aſtronomical obſervations.

Perhaps ſome gentlemen in the north of Devonſhire or Somerſetſhire may be induced to continue ſimilar operations, when they conſider with what little trouble they may be made, as the diſtances between any of the towns I have determined may ſuffice for a baſe.

XXII. *Experiments and Obſervations on the Matter of Cancer, and on the aërial Fluids extricated from animal Subſtances by Diſtillation and Putrefaction; together with ſome Remarks on ſulphureous hepatic Air.* By Adair Crawford, *M. D. F. R. S.*

Read June 17, 1790.

THERE are ſeveral varieties in the colour and conſiſtence of the matter diſcharged by cancerous ulcers. It is in ſome caſes of a pale aſh colour; in others, it has a reddiſh caſt; and in many inſtances it has more or leſs of a brown tinge, ſometimes approaching nearly to black. Its conſiſtence is for the moſt part thin; but in the cancerous, as well as in other malignant ulcers, we frequently meet with a white ſordes, which cloſely adheres to the ſurface of the ſore, and which appears to be ſcarcely miſcible with water. In the ſame patient the appearance of the diſcharge is frequently varied by internal remedies, or by external applications; but if we except the temporary variations produced by accidental circumſtances, the cancerous ulcer is, in its advanced ſtage, very generally accompanied with a peculiar odour more highly fetid and offenſive than that which is emitted by other malignant ulcers.

It is well known, that the cancerous matter occaſions by its abſorption ſchirrous tumors of the lymphatic glands contiguous to the parts affected; and that it gradually corrodes the branches

branches of the larger blood-veſſels, which have a peculiar power of reſiſting the action of other purulent diſcharges.

Apprehending that ſome light might poſſibly be thrown upon the nature of cancerous diſeaſes, by enquiring into the properties of this ſubſtance, I procured a portion of it from a patient who had for ſeveral years been afflicted with a cancer in the breaſt. Having diffuſed it through pure water, I divided it into three parts, which were put into ſmall glaſs veſſels. To one of theſe I added a ſolution of vegetable fixed alkali; to the ſecond, a little concentrated vitriolic acid; and to the third, ſyrup of violets. By the vegetable fixed alkali no ſenſible change was produced: upon the addition of the vitriolic acid, the liquor in the ſecond glaſs acquired a deep brown colour, a briſk efferveſcence took place, and at the ſame time the peculiar odour of the cancerous matter was greatly encreaſed, and diffuſed itſelf to a conſiderable diſtance through the ſurrounding air. The ſyrup of violets communicated to the liquor in the third glaſs a faint green colour.

The cancerous matter uſed in theſe experiments had a browniſh caſt. It had been imbibed by cotton, and kept for ſome days before the trials were made.

Mr. GEBER has ſhewn, that animal ſubſtances upon their firſt putrefaction do not efferveſce with acids; that, after the proceſs has continued for ſome time, a manifeſt efferveſcence takes place; and that this effect again diſappears before the putrefaction has ceaſed.

Suſpecting that the efferveſcence in the preceding experiment might have ariſen from a change which the matter underwent, in conſequence of its having been kept ſome days before the trial was made, I repeated the experiment with a portion of reddiſh matter recently obtained from a cancerous penis. Upon

the addition of the acid, the liquor, as before, acquired a brown colour, its fetor was much increaſed, and a manifeſt effervescence took place, although it was not ſo conſiderable as in the former inſtance. A portion of the ſame matter diffuſed through diſtilled water communicated a blue tinge to tincture of litmus, and a greeniſh caſt to ſyrup of violets.

It is proper to obſerve, that when ſyrup of violets was mixed with portions of cancerous matter from a variety of different ſubjects, the change produced was in ſome caſes ſcarcely perceptible; but in every inſtance the preſence of an alkali was detected by dipping into the matter a ſlip of paper that had been previouſly tinged blue by tincture of litmus, and afterwards ſlightly reddened by acetous acid. The red colour was invariably in the courſe of a few minutes aboliſhed, and the blue reſtored.

The cancerous matter, as has been already remarked, acquired, upon the addition of the vitriolic acid, a brown hue. It is well known, that this acid, when it is highly concentrated, communicates a brown or black colour to all animal and vegetable ſubſtances. Being deſirous of learning whether the change which took place upon the addition of the acid to the cancerous matter in this experiment, was different from that which would be produced by the ſame acid in other animal ſubſtances, and particularly in recent healthy pus; I took equal quantities of the latter, and of aſh-coloured cancerous matter, and having diffuſed each of them through thrice its weight of diſtilled water, I added to them equal quantities of concentrated vitriolic acid; the weight of the acid being nearly the ſame with that of the matter uſed in the experiment. The mixture containing the pus acquired from the acid a faint brown colour; but that which contained the cancerous matter, was

ſuddenly changed to a deep brown, approaching to black. When theſe mixtures were diluted with about twice their weight of diſtilled water, the brown tinge of the former entirely diſappeared; but the latter ſtill retained its brown colour, although it was ſomewhat fainter than it had been upon the firſt addition of the acid.

The aërial fluid which was diſengaged in the foregoing trials from the matter of cancer, by the vitriolic acid, appeared from its odour to have a nearer reſemblance to hepatic than to any other ſpecies of air. As it ſeemed, from its ſenſible qualities, to be a very active, and probably a deleterious principle, I endeavoured more particularly to enquire into its nature, and to compare it with common hepatic air. But before I relate the trials which were made with that view, it may not be improper briefly to mention the characters by which common hepatic air is diſtinguiſhed.

It has a ſmell reſembling that of rotten eggs; it is inflammable, and during its combuſtion in the open air, ſulphur is depoſited; it communicates a black colour to ſilver and copper, and a browniſh tinge to lead and iron; it is ſoluble in water, and when a ſolution of nitrated ſilver is dropped into water impregnated with it, the mixture becomes turbid, and a dark-coloured precipitate falls to the bottom; by the addition of the nitrated ſilver, the odour of the hepatic air is rendered much fainter; and it is entirely deſtroyed by concentrated nitrous, or by dephlogiſticated marine acid.

To determine whether the aërial fluid contained in the cancerous matter poſſeſſed theſe properties, a portion of this ſubſtance was diffuſed through diſtilled water. The mixture being filtered, a ſmall quantity of nitrated ſilver was dropped into it. In a little time, an aſh-coloured cloud was produced,

which

which ſoon afterwards acquired a browniſh purple hue, and at the end of two hours the colour of the mixture was changed to a deep brown. The fetid ſmell was now rendered much fainter than that of a ſimilar mixture of cancerous matter, and of diſtilled water, to which nitrated ſilver had not been added. When a little concentrated nitrous acid was dropped into the mixture which had been thus altered by the addition of nitrated ſilver, a ſlight efferveſcence took place, the brown hue was inſtantly changed to an orange colour, and the fetid ſmell was aboliſhed. The fetor was likewiſe entirely deſtroyed, when dephlogiſticated marine acid was added either to cancerous matter in its ſeparate ſtate, or to a portion of that ſubſtance which had been previouſly mixed with nitrated ſilver.

By the foregoing properties the cancerous virus is diſtinguiſhed from common pus: for when dilute vitriolic acid is added to common pus, no efferveſcence is produced; and when a ſolution of nitrated ſilver is dropped into this ſubſtance previouſly diffuſed through diſtilled water, the mixture does not acquire a brown colour; nor does any ſenſible precipitation take place for ſeveral hours. It appeared, however, that when the laſt experiment was repeated with matter obtained from a venereal bubo, the mixture upon the addition of the nitrated ſilver became ſlightly turbid, and, at the end of two hours, it acquired a browniſh caſt. The ſame effects were perceived when the trial was made with matter obtained from a carious bone. But in theſe inſtances the precipitation was much leſs conſiderable than that which was produced by the cancerous matter.

I next endeavoured to procure, in its ſeparate ſtate, a portion of the air which is extricated from the matter of cancer by the vitriolic acid. With this intention a quantity of reddiſh cancerous matter was mixed in a ſmall proof, with about

thrice its weight of diſtilled water. To this mixture a little vitriolic acid was added; upon which an effervescence took place, and the air that was diſengaged was received in a phial over mercury. When one half of the mercury was expelled from the phial, the latter was inverted over diſtilled water, and the portion of the mercury that remained in it being ſuffered to deſcend, and the water to riſe into its place, the phial was cloſely corked. The air and water were then briſkly agitated together, and the phial being a ſecond time inverted over diſtilled water, the cork was removed; when it appeared by the heighth to which the water roſe, that a part of the air had been abſorbed. The water contained in the phial was now found to be ſtrongly impregnated with the odour of the cancerous matter, and a little nitrated ſilver being dropped into it, a purpliſh cloud, inclining to red, was produced. It is proper to obſerve, that the change of colour upon the addition of the nitrated ſilver, in this experiment, was at firſt ſcarcely perceptible; but in the courſe of a few minutes it became very diſtinct. As it might perhaps be doubtful, whether this alteration would not be produced in the nitrated ſilver by expoſure to the air alone, the colour of the mixture was compared with that of a ſimilar mixture of nitrated ſilver and of pure diſtilled water, which had remained expoſed to the open air for an equal length of time. Although a ſlight change of colour was produced in the latter inſtance, yet it was much leſs conſiderable than that which took place in the former.

In the above recited experiment, the air came over mixed with the common air that was contained in the proof. The quantity of aërial fluid that can be thus extricated by the addition of the acid without the aſſiſtance of heat, is not very conſiderable.

If

If heat be applied, a larger portion of fetid air, having the odour of cancerous matter, may be diſengaged; but in that caſe it will be found to be mixed with vitriolic acid air.

With a view to obtain the former of theſe fluids in as pure a ſtate as poſſible, the experiment was repeated in the following manner. A portion of the cancerous virus, diffuſed through diſtilled water, was introduced into a ſmall proof; a little vitriolic acid was added; the veſſel was filled with diſtilled water, and a crooked tube, alſo filled with that fluid, was fixed to its neck. The extremity of the tube being then introduced into the mouth of an inverted bottle containing water, and the flame of a candle being applied to the bottom of the proof, a quantity of air was expelled, which was received in the bottle. This air, when it was firſt diſengaged, roſe in the form of white bubbles; it had a very fetid ſmell, ſimilar to that of the cancerous matter; and the water which was impregnated with it occaſioned a dark-brown precipitate in a ſolution of nitrated ſilver. The crooked tube being ſeparated from the proof, a very offenſive white vapour, reſembling in its odour the air extricated during the experiment, aroſe from the mixture, and continued to aſcend for nearly half an hour. When to a portion of this ſmoking liquor, previouſly filtered, a little concentrated nitrous acid was added, the fetid ſmell was entirely deſtroyed, a ſlight efferveſcence took place, and a flaky ſubſtance that floated through the mixture was diſengaged.

The foregoing experiments prove, in general, that the fetid odour of the matter of cancer is increaſed by the vitriolic, but entirely deſtroyed by the concentrated nitrous and dephlogiſticated marine acids; that the aërial fluid, which is diſengaged by the vitriolic acid, is ſoluble in water; and that the ſolution

tion depoſits a reddiſh brown precipitate upon the addition of nitrated ſilver. Whence it follows, that the cancerous matter contains a principle which has many of the properties of hepatic air, and which may perhaps not improperly be termed animal hepatic air.

It has moreover been ſhewn, that the matter of cancer is impregnated with an alkali which is in ſuch a ſtate as to change the colour of vegetable tinctures. I had very little doubt that this was the volatile alkali: for it is well known, that putrid animal ſubſtances frequently abound with that ſalt; but have never, I believe, been found to contain a fixed alkali in a diſengaged ſtate. With a view, however, more deciſively to determine this point, I tried the following experiment. A quantity of cancerous matter, diffuſed through diſtilled water, was introduced into a glaſs retort to which a receiver was adapted. The mixture was ſlowly diſtilled by means of a ſand heat; and a ſmall quantity of the liquor which came over into the receiver being poured into an infuſion of Brazil wood, inſtantly imparted to it a deep red colour.

Hence it clearly appears, that the alkali contained in the cancerous matter was the volatile, becauſe it was ſeparated by diſtillation with a heat which did not exceed that of boiling water.

It ſeemed extremely probable, that the above-mentioned alkali was united to the aërial fluid with which the matter of cancer is impregnated. Of the truth of this fact I was perſuaded by obſerving, that the ſmell of the cancerous matter was greatly increaſed by the addition of the vitriolic acid: for I could ſcarcely avoid concluding, that this phænomenon aroſe from an union between the acid and alkali, in conſequence of which the odoriferous principle was extricated by a ſuperior attraction.

attraction. This concluſion will be confirmed by experiments to be recited in the ſequel, which prove, that the volatile alkali is capable of entering into a chemical combination with the aërial fluid contained in the matter of cancer.

Of the air extricated from cancerous matter, and from other animal ſubſtances, by diſtillation.

A portion of matter from a cancerous breaſt was diffuſed through diſtilled water, and introduced into a ſmall coated glaſs retort, which was gradually expoſed to heat in a ſand bath till the bottom of the retort became red-hot. The neck of the latter was introduced below an inverted jar filled with water, and a quantity of air was received in the jar, which was found to conſiſt of the common air contained in the retort. Two meaſures of it, mixed with one of nitrous air, occupied the ſpace of a little leſs than two meaſures. This portion of air was ſtrongly impregnated with the peculiar ſmell of the cancerous matter.

The heat continuing to increaſe, the water began to boil, and a large quantity of aqueous vapour aroſe; which, as ſoon as it came into contact with the common air, produced a white ſmoke. The ſmell that was now perceived was remarked by thoſe who were preſent to be ſimilar to that of freſh animal ſubſtances when they are boiled. The aqueous vapour in this part of the proceſs was not mixed with any permanently elaſtic fluid.

When the greater part of the water was evaporated, the jar containing the firſt portion of air was removed, and the neck of the retort was introduced beneath an inverted veſſel filled with mercury. Soon after this, a conſiderable quantity of air, having a fetid ſmell ſimilar to that of burned bones, was extri-

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Of the air extricated from cancerous matter, and from other animal ſubſtances, by diſtillation.

A portion of matter from a cancerous breaſt was diffuſed through diſtilled water, and introduced into a ſmall coated glaſs retort, which was gradually expoſed to heat in a ſand bath till the bottom of the retort became red-hot. The neck of the latter was introduced below an inverted jar filled with water, and a quantity of air was received in the jar, which was found to conſiſt of the common air contained in the retort. Two meaſures of it, mixed with one of nitrous air, occupied the ſpace of a little leſs than two meaſures. This portion of air was ſtrongly impregnated with the peculiar ſmell of the cancerous matter.

The heat continuing to increaſe, the water began to boil, and a large quantity of aqueous vapour aroſe; which, as ſoon as it came into contact with the common air, produced a white ſmoke. The ſmell that was now perceived was remarked by thoſe who were preſent to be ſimilar to that of freſh animal ſubſtances when they are boiled. The aqueous vapour in this part of the proceſs was not mixed with any permanently elaſtic fluid.

When the greater part of the water was evaporated, the jar containing the firſt portion of air was removed, and the neck of the retort was introduced beneath an inverted veſſel filled with mercury. Soon after this, a conſiderable quantity of air, having a fetid ſmell ſimilar to that of burned bones, was extri-

cated. This aërial fluid was mixed with a yellow empyreumatic oil. A portion of it being agitated with water was found to be partly imbibed by that fluid; and nitrated ſilver, dropped into the water thus impregnated, produced a reddiſh precipitate.

One meaſure of the air, obtained in the foregoing experiment, being mixed over mercury with an equal bulk of alkaline air, the volume of the mixture was found gradually to decreaſe; and, at the end of three hours, the air in the tube occupied the ſpace of only one meaſure and two tenths. An oily depoſit was now made upon the inner ſurface of the tube. At the expiration of eight days, the interior ſurface of the tube was covered with ſlender films, which had a yellowiſh caſt, and which were irregularly ſpread upon it. The upper ſurface of the mercury within the tube was corroded; in ſome places it had a reddiſh burniſhed appearance; in others, it was changed into an aſh-coloured powder, interſperſed with brown ſpots. The tube was now removed from the mercury, and the air that remained in it had a ſtrong fetid ſmell, reſembling that of burned bones.

It has been already obſerved, that before the water was entirely evaporated, the vapour had loſt the odour of the cancerous matter, and had acquired that of animal ſubſtances recently boiled. Hence it appears, that the matter upon which the peculiar ſmell of cancerous ulcers depends, is a very volatile ſubſtance, for it eſcaped at the beginning of the proceſs. It alſo appears, that this volatile ſubſtance, which is probably the active principle in the matter of cancer, is not changed, by ſimple expoſure to heat, into a permanently elaſtic fluid; for the air that eſcaped at the beginning of the proceſs, although it ſmelled ſtrongly of the cancerous matter, was found by Dr.

PRIESTLEY's teſt to be as pure as common air; and it was evident, that the aqueous vapour which came over in the middle of the proceſs was not mixed with any permanently elaſtic fluid; becauſe, when this vapour was received in an inverted bottle filled with mercury, it was condenſed into water, without any admixture of air. Indeed, if the odoriferous principle in the matter of cancer conſiſt of volatile alkali combined with animal hepatic air, it could not be expected that it ſhould acquire a permanently elaſtic form by ſimple expoſure to heat; becauſe when alkaline and animal hepatic air unite together, they form a non-elaſtic ſubſtance that condenſes upon the inner ſurface of the veſſel in which they are mixed.

To diſcover whether other animal ſubſtances yield an aërial fluid, ſimilar to that which was extricated in the foregoing experiment from the matter of cancer by means of heat, a portion of the fleſh of the neck of a chicken was introduced into a ſmall coated glaſs retort, which was gradually expoſed to heat in a ſand bath till it became red-hot. A thin phlegm, of a yellowiſh colour, firſt came over: this was ſoon ſucceeded by a yellow empyreumatic oil, and at the ſame time a permanently elaſtic fluid, having an odour reſembling that of burned feathers, began to be diſengaged. A ſlip of paper, tinged with litmus and reddened by acetous acid, being held over this fluid, became blue. The neck of the retort was now introduced below an inverted jar filled with mercury, and a conſiderable quantity of air, together with a fetid empyreumatic oil, were received in the jar. This air was highly inflammable: it had a very fetid odour. When a bottle, containing a portion of it, was agitated with diſtilled water, nearly one-half of it was abſorbed. The reſidue was inflammable, and burned firſt with a ſlight exploſion, and afterwards with a blue lambent flame. A little nitrated ſilver being dropped into the

water with which the air had been agitated, the mixture inſtantly acquired a reddiſh brown colour; after ſome time it became turbid, and a brown precipitate fell to the bottom. When two meaſures of the air, extricated in this experiment, were mixed with one of alkaline air, they occupied the ſpace of a little more than one meaſure and an half. A ſecond meaſure of alkaline air being added, and the airs being ſuffered to remain together for three days, at the end of that time the reſidue occupied the ſpace of two meaſures and one-eighth. Soon after they were mixed, an oily fluid, of a pale colour, was depoſited on the internal ſurface of the jar. At the end of the third day this ſubſtance had acquired a light olive colour. It was collected in globules, irregularly diſtributed over the interior ſurface of the jar. Theſe globules were nearly of a ſolid conſiſtence. When the jar was removed from the mercury, the air contained in it at firſt ſmelled ſtrongly of volatile alkali. After a little time the ſmell of the alkali diſappeared, and the odour of empyreumatic oil was diſtinctly perceived. A ſmall quantity of diſtilled water, which was now agitated in the jar, acquired a brown colour, but did not entirely diſſolve the viſcid ſubſtance that adhered to its ſurface. The water, thus coloured, was divided into two portions. To one of theſe was added a little ſtrong vitriolic acid, by which the ſmell was exalted, and a ſlight efferveſcence was produced. Concentrated nitrous acid being added to the other portion, the ſmell and colour were deſtroyed, and a briſk efferveſcence took place.

When a portion of the ſolid ſubſtance that adhered to the interior ſurface of the jar was ſeparated, it felt viſcid and adheſive between the fingers, and ſmelled ſtrongly of empyreumatic oil. A little ſpirit of wine being introduced into the

jar, this viſcid ſubſtance was diſſolved; the ſpirit acquired a yellow colour and empyreumatic ſmell, and upon adding to it diſtilled water the mixture became whitiſh and ſlightly turbid.

I next examined the air extricated from putrid veal by diſtillation. A portion of the latter ſubſtance being introduced into a coated glaſs retort was expoſed to a red heat, and the air diſengaged was received in a jar over mercury. This aërial fluid was found to poſſeſs nearly the ſame properties with that which was obtained in the preceding experiments. It was very inflammable; about one-half of it was ſoluble in diſtilled water. The water, thus impregnated, became turbid upon the addition of nitrated ſilver, and a brown precipitate fell to the bottom. To another portion of diſtilled water ſaturated with this fluid, dephlogiſticated marine acid being added, the fetid ſmell was deſtroyed, a briſk effervеſcence took place, and a whitiſh gelatinous ſubſtance was ſeparated. This ſubſtance being evaporated to dryneſs, became black upon the addition of concentrated vitriolic acid. When a quantity of the air obtained in the experiment was agitated with diſtilled water until no more was abſorbed, the reſidue took fire upon the application of an ignited body, and burned with a lambent flame. It is proper to obſerve, that the air extricated from the putrid veal had leſs of the empyreumatic ſmell than that which was diſengaged from freſh animal ſubſtances. Its odour indeed was nearly ſimilar to that of animal ſubſtances in a ſtate of putrefaction.

We learn from theſe experiments that the aërial fluids, which are extricated from freſh as well as from putrid animal ſubſtances by diſtillation, have nearly the ſame properties with that which is diſengaged, by a ſimilar proceſs, from the matter of cancer. Each of them appears to conſiſt of two diſtinct fluids; one of which is ſoluble, and the other inſoluble, in

water. The portion that is inſoluble burns with a lambent flame, and has all the characters of heavy inflammable air; whereas the ſoluble part reſembles the fluid which is extricated from cancerous matter by the vitriolic acid: it has a fetid odour, it decompoſes nitrated ſilver, combines with cauſtic volatile alkali, and poſſeſſes many of the properties of common hepatic air.

There are ſeveral particulars, however, in which the animal and common hepatic air materially differ from each other. Although they are both fetid, yet their odours are not exactly ſimilar. When common hepatic air is decompoſed by the concentrated nitrous or dephlogiſticated marine acid, ſulphur is ſeparated; but when animal hepatic air is decompoſed by theſe acids, a white flaky matter is diſengaged which is evidently an animal ſubſtance, becauſe it becomes black by the addition of concentrated vitriolic acid. Sulphur is moreover ſeparated during the combuſtion of common hepatic with atmoſpherical air; but when the air from animal ſubſtances is burned with atmoſpherical air, no precipitation of ſulphur takes place. Indeed, that animal hepatic air does not contain ſulphur will be apparent from the following experiment.

Equal parts of pure air and of air extricated from freſh beef by diſtillation, were fired by the electric ſhock in a ſtrong glaſs tube over mercury. A little diſtilled water was then introduced through the mercury into the tube, and was agitated with the air which it contained. A portion of this water being filtered, and a ſmall quantity of muriated barytes being dropped into it, the mixture remained perfectly tranſparent. Hence it appears, that the air extricated from freſh beef by diſtillation does not contain ſulphur; for, if it had contained that ſubſtance, the ſulphur, by its combuſtion with the pure air, would have been

changed into the vitriolic acid, and the muriated baryte would have been decompoſed.

I frequently repeated the preceding experiment with the air extricated, by diſtillation, from the putrid as well as from the freſh muſcular fibres of animals; but I could not, in any inſtance, diſcover the leaſt veſtige of the vitriolic acid.

The following experiments were made with a view more accurately to analyſe the airs which are diſengaged from animal ſubſtances by heat, and to determine the products reſulting from the union of theſe fluids with pure air.

About an ounce of the lean of freſh mutton was introduced into a ſmall coated glaſs retort, and expoſed to a red heat. The air that was extricated towards the end of the diſtillation was received over mercury; and ſoon after its production, being agitated with water, very nearly one half of it was abſorbed. A ſimilar experiment being made with the air diſengaged towards the middle of the diſtillation, the part of it which was ſoluble in water was found to be to the part not ſoluble in that fluid as 2 to 3. Having ſuffered a ſeparate portion of the air diſengaged towards the end of the diſtillation to remain over mercury for ſeven hours, it was found gradually to diminiſh in bulk, and a fluid, which had the colour and the odour of a thin empyreumatic oil, was collected at the bottom of the jar *. The air being now agitated with water, only one-eighth of it was abſorbed. Hence it appears, that a portion of the air, extricated from animal ſubſtances by heat, reſembles a ſpecies of hepatic air which was firſt diſcovered by Mr. KIRWAN, and which exiſts in an intermediate ſtate between the aërial and the vaporous; this fluid not being permanently elaſtic like

* The above-mentioned appearance is not conſtant. The air when placed over mercury ſometimes diminiſhes, and at other times it retains its original bulk. I have not as yet diſcovered the cauſe of this difference.

air,

air, nor immediately condenſed by cold like vapour, but gradually aſſuming the non-elaſtic form, in conſequence probably of the tendency of its ſeveral parts to unite with each other.

The air produced in the foregoing experiment rendered lime-water turbid; it therefore contained a quantity of fixed air; and towards the end of the diſtillation a little volatile alkaline air came over, agreeably to the obſervation of M. BERTHOLLET: for, when a portion of the air received during this part of the proceſs was mixed with an equal quantity of marine acid air, a white vapour was produced, and a diminution of about one twenty-fifth of the whole took place.

I endeavoured, by the following experiment, to aſcertain the proportion of fixed air contained in the aërial fluid which is diſengaged from the lean of animal ſubſtances by heat.

A quantity of air, extricated from the lean of freſh mutton, was received over mercury in a large phial which had a narrow neck. When the phial was a little more than half filled, the remaining portion of the mercury was diſplaced by introducing water that had been previouſly boiled. The phial being then cloſely corked, the air and water were briſkly agitated together; and the liquor, thus impregnated with the ſoluble part of the animal air, was put into a proof, to the bottom of which heat was applied. By this means a portion of the air was again diſengaged, which was received in a tube inverted over mercury. The proceſs was continued till the liquor in the proof no longer rendered lime-water turbid. As the air received in the tube contained the fixed air that had been extricated from the liquor, together with a quantity of common air expelled from the proof, it was a ſecond time agitated with water; and the exact meaſure of the fixed air was known by the portion which the water imbibed. The fixed air, thus aſcertained, being compared with the entire quantity of air that had been originally abſorbed, it appeared,

that

that the former was to the latter in bulk as 1 to 4. One-fourth therefore of the volume of the ſoluble part of animal air conſiſts of fixed air, and the remaining three-fourths of hepatic, mixed with a very ſmall proportion of alkaline air *.

It appeared from the experiment, that animal hepatic air, when it was abſorbed by water, was not capable of being again diſengaged by a heat which raiſed the water to the boiling temperature; for, after the fixed air was expelled, the liquor in the proof was made to boil nearly half an hour, but no permanently elaſtic fluid could be diſengaged. The portion of the liquor which now remained had a faint yellow colour; it ſmelled ſtrongly of animal hepatic air, and depoſited a brown precipitate upon the addition of nitrated ſilver.

It appears, therefore, that the ſoluble part of the air which is diſengaged from the lean of animal ſubſtances by heat, conſiſts of three diſtinct fluids; of alkaline air, fixed air, and animal hepatic air. It ſeemed extremely probable, that theſe three aërial fluids, ſlowly combining together, formed the oily empyreumatic ſubſtance which was collected at the bottom of the jar, while the air was undergoing the diminution deſcribed above. In this concluſion I was confirmed by trials that were made with the empyreumatic oil that came over during the latter part of the diſtillation: for when it was examined by chemical teſts, ſoon after it was obtained, it was found to contain fixed air, volatile alkali, and animal hepatic air.

* It is proper to remark, that, in ſome experiments, the relative quantity of animal hepatic air was found to be leſs than that which has been ſtated above. I do not as yet know, with certainty, the cauſes to which this difference is to be attributed; but I believe it principally depends upon the period of the diſtillation in which the air is received, the degree of heat applied to the bottom of the retort, and the nature of the animal ſubſtance employed in the experiment.

I next endeavoured to determine the products which result from the combuſtion of pure air, with animal air, or with the compound aërial fluid extricated from the lean of animal ſubſtances by heat. With this intention I expoſed the lean of freſh mutton, in a ſmall coated glaſs retort, to a red heat. The air which was received over mercury towards the end of the diſtillation was divided into two ſeparate portions; one of which was agitated with water till the ſoluble part was abſorbed; the other was not agitated with that fluid. One meaſure of the former was introduced, over mercury, into a ſtrong glaſs tube adapted for the purpoſe of firing aërial fluids by the electric ſhock. This was mixed with one meaſure and an half of pure air. The portion of the tube occupied by the mixture was one inch and two-tenths. A ſmall ſhock being made to paſs through it, a violent exploſion took place, and the ſpace occupied by the reſidue was nine-tenths of an inch. The height of the mercury in the tube, previouſly to the combuſtion, was 4.8 inches. After the airs were fired, its height was 5.1 inches. Allowance being made for the difference of expanſion produced by this cauſe, it appeared, that the volumes of the airs, previouſly to the combuſtion, and ſubſequent to it, were as 100 to 75 nearly. The reſidue being agitated with water, ſix-tenths were abſorbed; and the portion which was thus abſorbed was found, by the precipitation which it produced in lime water, to be fixed air. Of the inſoluble remainder, five parts being mixed with five of nitrous air, a diminution of three parts took place; whence it follows, that one-fifth of the inſoluble reſidue was pure air.

The pure air which was uſed in this experiment had been previouſly agitated with water, to free it entirely from fixed air, and the inflammable air had undergone a ſimilar agitation.

It

It is therefore manifeſt, that, by the combuſtion of the pure and inflammable air in the foregoing trial, fixed air was produced; the phlogiſticated air, found in the reſidue, being that which was contained in the pure air before the inflammation took place.

I next examined the products reſulting from the combuſtion of pure air with that portion of the animal air which had not been previouſly agitated with water. One meaſure of this fluid, at the expiration of three-fourths of an hour after it had been obtained, was mixed over mercury with one meaſure and an half of pure air, and fired by the electric ſhock. The portion of the tube occupied by the mixture, previouſly to the deflagration, was one inch and $\frac{15}{100}$; after the deflagration, it occupied the ſpace of one inch and one-tenth. Being agitated with lime-water, very nearly one-third was abſorbed. A portion of the inſoluble reſidue was expoſed to a lighted taper, and burned with a faint blue flame *.

The dephlogiſticated air uſed in this experiment had been previouſly agitated with water, to free it entirely from fixed air. It was the pureſt dephlogiſticated air I had ever ſeen: for when one meaſure of it was mixed with one meaſure and nine-tenths of nitrous air, the reſidue occupied the ſpace of only one-fortieth of a meaſure. From the foregoing trial it was evident, that $1\frac{1}{2}$ parts of pure air were inſufficient to ſaturate one of the animal air that had not been previouſly agitated with water. The experiment was therefore repeated as follows. Two

* When I firſt made the above experiment, the reſidue did not appear to be inflammable. It had been tried by applying an inflamed ſlip of paper to the mouth of a phial which was filled with it; but, upon repeating the experiment, when the phial containing the reſiduary air was carried into a dark room, and an ignited wax taper was applied to its mouth, an evident inflammation took place.

parts of pure air being mixed with one of animal air, occupied .8 of an inch. The mixture being fired by the electrical ſhock, the reſidue ſtood at a little leſs than .5. When this reſidue was agitated with lime-water, it was almoſt wholly abſorbed. By a ſubſequent trial it was found, that nearly one-half of the animal air uſed in this experiment was ſoluble in water.

Hence it appears, that the quantity of pure air required to ſaturate the inſoluble part of the animal air is ſomewhat leſs than that required to ſaturate the compound fluid which had not been previouſly agitated with water. But the latter fluid has been ſhewn to conſiſt almoſt entirely of heavy inflammable, animal hepatic, and fixed air; and as the laſt of theſe is already ſaturated with pure air, it is manifeſt, that the above-mentioned difference muſt depend upon the animal hepatic air. Whence it follows, that the latter contains a large portion of the inflammable principle. From the quantity of fixed air produced in the laſt of the preceding experiments, there is, moreover, the utmoſt reaſon to believe, that the baſis of heavy inflammable forms one of the conſtituent parts of animal hepatic air.

When equal parts of pure and animal air were burned together, a conſiderable increaſe of bulk almoſt invariably took place; and when the proportion of the animal was to that of pure air as 21 to 15, the bulk of the mixture was increaſed one half. The air that remained after the combuſtion in the laſt mentioned experiments was inflammable: for a portion of it being introduced into a ſmall phial, and expoſed to a lighted candle, it firſt exploded, and then burned with a blue lambent flame.

Being deſirous of learning the cauſe of the increaſe of bulk in the foregoing experiments, the following trials were made.

Three meaſures of animal were mixed with two of pure air, and ſeveral ſtrong electrical ſhocks were made to paſs through the mixture, but it would not take fire. Half a meaſure of pure air was then added, and the mixture being fired, its bulk was encreaſed from .9 of an inch to 1 inch and .3.

Three meaſures of this reſiduary air were then mixed with three of pure air, and fired by the electric ſhock. The bulk of the mixture was reduced from 1 inch to .56. This being agitated with lime-water, two-thirds were abſorbed, and the remainder conſiſted almoſt wholly of pure air. From theſe facts it ſeems probable, that animal hepatic air conſiſts of a combination of heavy and light inflammable air; and that when it is fired with a quantity of pure air not ſufficient to ſaturate it, a portion of the animal air is reſolved into its elementary principles, in conſequence of which its bulk is encreaſed.

It was before obſerved, that three parts of animal mixed with two of pure air would not take fire. In ſome experiments it was found, that when the animal air was mixed with a ſtill ſmaller proportion of pure air, an increaſe of bulk was produced by the electric ſhock, although no deflagration took place; but when the electric ſhock was repeatedly taken through animal air *alone*, it did not in any inſtance, as far as I could perceive, produce the ſmalleſt increaſe of ſize.

I was next deſirous of learning whether an increaſe of ſize would be produced by making the electric ſhock paſs through a mixture of pure and alkaline air. Having firſt accidentally taken two or three ſmall ſhocks through a little alkaline air, and not obſerving a ſenſible augmentation of bulk, I then mixed it with an equal volume of pure air; and, as I ſuppoſed that no decompoſition had taken place, I was not apprehenſive

of an explosion. Contrary, however, to my expectation, the airs, when the electric shock was made to pass through them, entered rapidly into an union with each other. The jar which I held loosely in my hand, as it was inverted over the mercury, was carried obliquely upwards with great violence. Having broken the stand of the prime conductor in its passage, it forced its way through the cylinder of the electrical machine, which it shivered to a thousand pieces.

I afterwards repeated this experiment with a very strong apparatus, the jar being pressed down by a plate of iron, for the purpose of retaining it in its place.

It appeared, that when the alkaline and pure air were immediately mixed together, and a small shock was made to pass through them, they would not take fire; but when three or four shocks were previously taken through the alkaline air, and the latter was afterwards mixed with an equal bulk of pure air, they exploded with great violence. The residue, having cooled to the temperature of the surrounding air, was reduced to half the original bulk of the mixture. Of this residue one-fixth was undecomposed alkaline air. The remainder was phlogisticated air.

Of the products which result from the combustion of sulphureous hepatic with pure air.

The hepatic air employed in the following experiments was procured, agreeably to the method which Mr. KIRWAN has recommended, by adding marine acid to an artificial combination of sulphur and iron. Three measures of the air thus obtained were mixed in a strong glass tube over mercury, with four of pure air, and fired by the electric shock.

The pure air was previouſly agitated with lime-water to free it from fixed air, and a portion of the hepatic air, having been likewiſe agitated with lime-water, was found not to occaſion any precipitation in that fluid. The airs were reduced by the exploſion to one-fourth of their original bulk. The reſidue was then transferred over mercury into a ſlender graduated tube, and diſtilled water being admitted, eight-tenths were abſorbed. To a portion of this water, when filtered, vitriolated ſilver was added, which inſtantly occaſioned a copious precipitate. To a ſecond portion was added muriated barytes, which occaſioned a ſlight white precipitate not re-diſſolvable in a large quantity of water; lime-water being added to a third portion, did not produce any ſenſible precipitation. From the laſt fact it does not follow, that no fixed air exiſted in the reſidue, becauſe the marine acid, which it evidently contained, would diſſolve the calcareous earth of the lime-water. As a great diminution, however, reſulted from the combuſtion; and as it appeared, from chemical teſts, that the reſidue was moſtly compoſed of marine and vitriolic acid airs, it is manifeſt, that, if any fixed air was produced, its quantity muſt have been very inconſiderable.

It has been already obſerved, that a ſlight precipitation took place upon the addition of the muriated barytes. The precipitate was much more conſiderable when, upon repeating the experiment, the reſidue after the exploſion was not tranſferred into a graduated tube previouſly to the admiſſion of the diſtilled water; but the latter was immediately introduced into the veſſel in which the airs were fired. The reaſon of this difference is evident. The ſlight precipitate by the muriated barytes, in the firſt inſtance, depended upon the exiſtence of a ſmall quantity of vitriolic acid in an aërial form, or in the

ſtate

ſtate of volatile vitriolic acid, which was transferred together with the phlogiſticated and marine acid air into the ſecond tube; but the greater part of the vitriolic acid produced by the combuſtion adhered, in a fixed ſtate, to the ſurface of the tube in which the airs were fired; and therefore, when the diſtilled water was immediately introduced into this tube, a copious precipitate was depoſited upon the addition of muriated barytes.

Hence it appears, that when pure air and ſulphureous hepatic air, obtained from artificial pyrites by the marine acid, are fired together in the above proportions, the products are fixed vitriolic acid, together with a ſmall quantity of the volatile vitriolic, and marine acids, in an aërial form. The reſidue, which the diſtilled water did not abſorb, was the phlogiſticated air that exiſted in the pure air previouſly to the combuſtion.

From ſubſequent trials it appeared, that, when hepatic and pure air were fired in equal bulks, the reſidue had a ſtrong odour of volatile vitriolic acid, and moreover contained a ſmall proportion of undecompoſed hepatic air. Theſe facts ſeem to prove, that the converſion of ſulphur into volatile or fixed vitriolic acid depends upon the quantity of pure air with which it is ſupplied.

The marine acid air, found in this experiment, did not appear to form one of the conſtituent principles of the hepatic air, but to be merely diffuſed through it; for it was almoſt wholly ſeparated, by means of diſtilled water, from a different portion of the ſame air, which was placed in a tube inverted over mercury; the water having a ſtronger attraction to the marine acid than to the hepatic air.

By the following experiment I endeavoured to determine whether

whether vitriolic acid be produced by the combuſtion of hepatic with atmoſpherical air. One meaſure of hepatic air, obtained from artificial pyrites, was mixed over mercury with about ſix meaſures of atmoſpherical air, and fired by the electric ſhock. A copious precipitation of ſulphur took place, the remaining air was then agitated with diſtilled water, the latter was filtered, and muriated barytes was added, which produced a white precipitate not diſſoluble in a large quantity of water.

From this, and the foregoing experiment, it appears, that when ſulphureous hepatic is burned with atmoſpherical air, a part of the ſulphur is changed into vitriolic acid, and the reſt is precipitated; but when it is burned with a ſufficient quantity of pure air, the ſulphur is wholly converted into vitriolic acid. Agreeably to this concluſion, I have found that the odour of the volatile vitriolic acid conſtantly accompanies the combuſtion of hepatic with common air in open veſſels; and that when concentrated nitrous acid is added to water impregnated with hepatic air, the filtered liquor becomes turbid upon the addition of muriated barytes.

The quantity of pure air required to ſaturate ſulphureous hepatic air, does not appear to correſpond with the ſuppoſition that the laſt of theſe fluids conſiſts of ſulphur diſſolved in light inflammable air: for ſulphur, in order to its complete ſaturation, requires only 1.43 times its weight of pure air; but light inflammable air requires for its ſaturation at leaſt ſix times its weight of that fluid. The ſpecific gravity of hepatic air, as determined by Mr. KIRWAN, is nearly equal to that of pure air. If, therefore, one-ſixth of the weight of hepatic conſiſted of light inflammable air, that fluid would require for its ſaturation 2.26 times its bulk of pure air: for the portion

of

of it which conſiſted of light inflammable air would require a quantity of pure air equal in bulk to the hepatic; and the remaining portion, conſiſting of ſulphur, would require a quantity equal to 1.26 of the hepatic. The entire quantity of pure air would therefore be to that of the hepatic as 2.26 to 1. If the hepatic contained one-twelfth of its weight of light inflammable air, it would require for its ſaturation 1.64 of its bulk of pure air. But from the foregoing experiments it appears, that the quantity of pure air, neceſſary to ſaturate one meaſure of hepatic air, is only 1.33 meaſures. Hence it is probable, that this fluid does not conſiſt of ſulphur diſſolved in light inflammable air.

If we make allowance for the marine acid which was diffuſed through the hepatic air, it will be found, that the quantity of pure air required to ſaturate it is nearly the ſame with that which would be required to change an equal weight of ſulphur into vitriolic acid. Whence it may be inferred, agreeably to the opinion of Mr. KIRWAN, that hepatic air is ſulphur which has acquired an aërial form by the application of heat. This concluſion is, I think, confirmed by the following experiment.

A little pure ſulphur was introduced into an inverted tube, which had been previouſly filled with mercury, and the flame of a candle was applied to the extremity of the tube. In a ſhort time a permanently elaſtic fluid was produced, which was found to have all the characters of hepatic air. It is probable, however, that ſome degree of moiſture is neceſſary to the ſucceſs of this experiment, becauſe the quantity of hepatic air which was thus obtained was not very conſiderable.

It has been already ſhewn, that an oily matter was produced by the union between fixed air, volatile alkali, and animal

hepatic

hepatic air. The following experiment proves, that a ſubſtance, which has very much the appearance of oil, is formed by the combination of *ſulphureous* hepatic air with fixed air and volatile alkali.

A quantity of impure hepatic air was obtained by adding vitriolic acid to common liver of ſulphur. When this fluid was agitated with lime-water, it produced a copious precipitation. It therefore contained a conſiderable proportion of fixed air. One meaſure of it was now introduced into a ſlender graduated tube, inverted over mercury, and was mixed with an equal bulk of alkaline air. As ſoon as the airs came into contact with each other, a white cloud was produced, the mercury began gradually to riſe in the tube, and at the end of ſix hours the air that remained occupied the ſpace of only one meaſure and one-third. The ſurface of the mercury within the tube firſt became black, and a part of it afterwards acquired a red colour reſembling cinnabar. In the courſe of the experiment, a yellowiſh oleaginous ſubſtance was depoſited upon the interior ſurface of the tube. This ſubſtance, in ſome parts of the ſurface, formed itſelf into globules; in others, it was extended into ramifications, having the reſemblance of trees in miniature, and it gradually aſſumed a deeper colour, till at length it acquired a greeniſh caſt. The ſubſtance, thus obtained, had a very fetid odour: it appeared to have a near reſemblance to an animal oil which had become green by putrefaction. It was, however, ſoluble in water, and the odour of the ſolution was increaſed by the vitriolic, and deſtroyed by the concentrated nitrous and dephlogiſticated marine acids.

Mr. CRUIKSHANK, who aſſiſted me in moſt of the foregoing experiments, and on whoſe accuracy I could place the greateſt reliance, examined, in my abſence, the red and black

powders which were formed by the action of the hepatic air upon the ſurface of the mercury, and found them to be æthiops mineral, and cinnabar.

Of the air extricated from animal ſubſtances by putrefaction.

In the beginning of July, 1789, about two ounces of veal, ſlightly putrid, was introduced into a large phial, which was filled with diſtilled water, and inverted over a quantity of the ſame fluid. At the end of three days a few bubbles of air had appeared at the bottom of the phial; the water had acquired a light brown colour, and emitted a fetid ſmell. At the expiration of ſeven days we could perceive that the quantity of air at the bottom of the phial was manifeſtly increaſed, although its progreſs was very ſlow. The water, by the diſſolution of a part of the veal, had now acquired the conſiſtence of a thin mucus, its brown colour was ſomewhat deepened, and it emitted a highly fetid ſmell. A little nitrated ſilver being dropped into a portion of this water, previouſly filtered, a dark brown precipitate was immediately produced. Lime-water, mixed with another portion of it, occaſioned an aſh-coloured precipitate; and when concentrated nitrous acid was added to a third portion, the fetid ſmell was deſtroyed, a ſlight effervescence took place, and a yellow flaky matter was diſengaged. At the end of ſeven weeks, a quantity of air, amounting to two and one-ſixth dram meaſures was collected in the phial. This air had a fetid odour. Being agitated with water, ſix-tenths of it was abſorbed. The reſidue extinguiſhed flame.

I next examined the air extricated from veal which was ſuffered to putrefy over mercury.

On

On the 28th of July, 1789, two drams and twenty-four grains of the lean of freſh veal was introduced into a narrow jar, which was filled with mercury, and inverted over that fluid. At the end of eight days the air, which was ſlowly extricated, had communicated a brown colour to the ſurface of the mercury. On the 13th of September, the quantity of air diſengaged was a little more than two ounce meaſures. This fluid had a very fetid ſmell. Two ſeparate portions of diſtilled water being ſaturated with it, the firſt, upon the addition of nitrated ſilver, depoſited a brown precipitate; and the laſt, when it was mixed with lime-water, produced a browniſh aſh-coloured cloud. A third portion of the air being ſtrongly agitated with diſtilled water, was reduced to one-ſixteenth of its original bulk. The reſidue extinguiſhed flame.

The veal which had remained ſo long in contact with the mercury had not loſt its firm texture. Its ſmell was putrid, but not very offenſive.

The quantity of elaſtic fluid collected in this experiment was much greater than in the preceding one; becauſe in the preceding experiment, although the putrefaction advanced more rapidly, yet the fixed and hepatic air were abſorbed by the water nearly as faſt as they were diſengaged from the putrid ſubſtance.

Hence it appears, that the aërial fluids, which are extricated from the muſcular fibres of animals by putrefaction, conſiſt of fixed and animal hepatic, mixed with a very ſmall proportion of phlogiſticated air *.

* It may be proper to remark, that I have obtained, by diſtillation from the green leaves of a cabbage, an aërial fluid, which, in moſt of its properties, reſembles animal hepatic air.

Of the effects produced by expoſing freſh animal ſubſtances to atmoſpherical, hepatic, and pure air.

Two tubes, of nearly the ſame ſize, were inverted over mercury. Into one of theſe was introduced common air, and into the other an equal bulk of hepatic air, obtained from liver of ſulphur by the vitriolic acid. Equal quantities of freſh veal, conſiſting of a mixture of muſcular fibres and of fat, and weighing each one dram, were then expoſed to theſe airs. At the end of three days the piece that was in contact with the common air had not altered its colour or conſiſtence, but ſmelled a little putrid. The colour of the fatty parts of the piece that was expoſed to the hepatic air was changed to a dark green, the muſcular fibres were cracked and ſhrivelled on the ſurface as if they had been ſeared with a hot iron, and the whole had acquired a ſoft conſiſtence.

Similar trials were made with two pieces of freſh veal, one of which was expoſed over mercury to common air, and the other to air extricated from putrid veal by diſtillation. The former in three days had not changed its appearance; the latter had become green round the edges, and was interſperſed with green ſpots. The ſurface of the mercury in the jar which contained the laſt had acquired a brown colour; whereas that of the mercury in the jar which contained the common air was clear and bright. The pieces of veal were ſuffered to remain in this ſituation for ſix weeks. After a few days had expired, *that* which was expoſed to the animal air did not appear to ſuffer any farther change. Its colour, which in the courſe of a week had become brown, continued unaltered, and no diſſolution took place. The air at the laſt was very fetid; it occaſioned

ſioned a copious precipitate in lime-water; it was highly inflammable, and burned with a blue lambent flame.

The piece, on the contrary, which was expoſed to the common air, did not, as has been already obſerved, ſo ſoon loſe its fibrous texture, nor ſo ſpeedily acquire a dark colour, as that which was in contact with the animal air. But the progreſs of its putrefaction did not appear to ſtop at the end of a few days, as in the latter inſtance. It advanced ſlowly, and at the expiration of ſix weeks a conſiderable part of the muſcular fibres had run down to a brown liquid. The air in which it was placed now occaſioned a copious precipitation in lime-water, and the brown liquid was found to be impregnated with animal hepatic and fixed air; the exiſtence of the latter being known by means of lime-water, and that of the former by its occaſioning a dark precipitate in a ſolution of nitrated ſilver, as well as by its fetid odour, which was increaſed by the vitriolic, and deſtroyed by the concentrated nitrous and dephlogiſticated marine acids.

The following experiment was made with a view to determine whether pure air accelerates the progreſs of putrefaction in animal ſubſtances.

In the month of December, 1789, equal portions of pure and of common air were introduced into two equal jars over mercury, in each of which was placed about two drams of freſh beef. At the end of a week, the beef which was expoſed to the pure air had become highly putrid; but very little change was produced in that which was expoſed to the common air.

The facts which have been aſcertained by the preceding experiments, appear to lead to the following concluſions reſpecting the proceſs of putrefaction in the lean of animal ſubſtances.

The muſcular fibres of animals contain fixed and phlogiſticated air, the inflammable principle in the ſtate of heavy and of light inflammable air, and a ſubſtance which, by means of heat or of putrefaction, is capable of being converted into animal hepatic air*. When the muſcular fibre, after the death of the animal, is expoſed to the pure air of the atmoſphere; the latter, by a ſuperior attraction, combining with the heavy inflammable air, produces fixed air, and at the ſame time furniſhes the quantity of heat neceſſary to the formation of animal hepatic air. The coheſion of the fibre being thus deſtroyed, the fixed, as well as the light inflammable and phlogiſticated air, which enter into its compoſition, are diſengaged, and the two latter fluids uniting with each other produce the volatile alkali.

The alterations which take place in putrefaction are in moſt reſpects ſimilar to thoſe which ariſe from deſtructive diſtillation. By expoſure to heat the fixed air of the animal fibre is extricated, hepatic air and volatile alkali are produced, and the inflammable principle not coming into contact with the pure air of the atmoſphere, is raiſed in the form of heavy inflammable air.

I have found, that the fetid odour of animal hepatic air is deſtroyed by mixing it with pure air, and ſuffering it to remain in contact with that fluid for ſeveral weeks. When it was placed in this ſituation, it acquired an odour which was not exactly ſimilar to any that I had ever before perceived, but which bore ſome reſemblance to that of inflammable air obtained by diſſolving iron in ſpirit of vitriol.

* It is ſcarcely neceſſary to obſerve, that the exiſtence of fixed, inflammable, and phlogiſticated air in animal ſubſtances, and the compoſition of volatile alkali, were diſcovered before I began to give particular attention to this ſubject.

The peculiar ſmell of animal hepatic air is likewiſe deſtroyed by agitating it with vinegar, or with the concentrated vitriolic acid. But the fluids which moſt ſpeedily produce this effect, are the concentrated nitrous and dephlogiſticated marine acids; and theſe fluids are known to abound with pure air. It is therefore extremely probable, that this alteration depends upon an union between the pure air of the latter ſubſtances and the animal hepatic air, or ſome of its conſtituent parts.

It appears from the experiments which have been recited above, that in cancerous and other malignant ulcers, the animal fibres undergo nearly the ſame changes which are produced in them by putrefaction, or by deſtructive diſtillation. The purulent matter prepared for the purpoſe of healing the ulcer is, in ſuch caſes, mixed with animal hepatic air and volatile alkali. The compound formed by the union of theſe ſubſtances, which may perhaps not improperly be termed hepatiſed ammonia, decompoſes metallic ſalts, and acts upon metals: for we have ſeen, that when it was placed in a jar over mercury for ſeveral days, the ſurface of the mercury acquired a black colour; and that it inſtantly occaſioned a dark precipitate in a ſolution of nitrated ſilver. Theſe facts ſeem to afford an explanation of the changes produced in metallic ſalts, when they are applied to malignant ulcers. The volatile alkali combines with the acid of the metallic ſalt, and the animal hepatic air revives the metal, either by imparting to it the inflammable principle, or by uniting with the pure air which the calx is ſuppoſed to contain. The metal, thus revived, is probably in ſome caſes again corroded by the hepatiſed ammonia, which communicates to it a black colour. Thus we may account for the dark incruſtation frequently formed upon the

tongue

tongue and internal fauces, when venereal ulcers of the throat are waſhed with a ſolution of corroſive ſublimate. And hence alſo the dark tinge which is frequently communicated by ill-conditioned ulcers to poultices made with a ſolution of ſugar of lead. The action of the hepatiſed ammonia likewiſe explains the reaſon why the probes are frequently corroded when they are introduced into ſinuous ulcers, or applied to the ſurfaces of carious bones. To the ſame cauſe it is probably owing, that poliſhed metallic veſſels are quickly tarniſhed, when they are expoſed to the effluvia of putrid animal ſubſtances.

From the foregoing experiments it moreover appears, that animal hepatic air imparts to the fat of animals recently killed a green colour; that it renders the muſcular fibres ſoft and flaccid, and increaſes the tendency to putrefaction. It is therefore a ſeptic principle; and hence it is extremely probable, that the compound of this fluid with volatile alkali, which is found in the matter diſcharged by the open cancer, produces deleterious effects: for although the miſchief in cancerous ulcers ſeems principally to depend upon a morbid action of the veſſels, whence the unhealthy ſtate of the matter diſcharged by ſuch ulcers is ſuppoſed to derive its origin, yet from the corroſion of the coats of the larger blood-veſſels, and the obſtructions in the contiguous glands, there can be little doubt that this matter aggravates the diſeaſe. The experiments recited above appear to prove, that the hepatiſed ammonia is the ingredient which communicates to the cancerous matter its putrid ſmell, its greater thinneſs, and, in a word, all the peculiar properties by which it differs from healthy pus.

From theſe conſiderations it was inferred, that a medicine which would decompoſe the hepatiſed ammonia, and deſtroy the fetor of the animal hepatic air, without at the ſame time

increaſing

increaſing the morbid action of the veſſels, would be productive of ſalutary effects. The nitrous acid does not deſtroy the fetor of hepatic air, unleſs it be highly concentrated; and in this ſtate it is well known that it ſpeedily corrodes animal ſubſtances. But the fetor of hepatic air quickly diſappears when it is mixed with the dephlogiſticated marine acid, even though the latter be ſo much diluted with water as to render it a very mild application. I have found that this acid, diluted with thrice its weight of water, gives but little pain when it is applied to ulcers that are not very irritable; and in ſeveral caſes of cancer it appeared to correct the fetor, and to produce a thicker and more healthy pus. It is proper, however, to remark, that other caſes occurred in which it did not ſeem to be attended with the ſame ſalutary effects. Indeed, ſome cancerous ulcers are ſo extremely irritable, that applications which are at all of a ſtimulating nature cannot be ventured upon with ſafety. And hence if the obſervations, which I have made on the efficacy of this acid as an external application, ſhould be confirmed by future experience, it muſt be left to the judgement of the ſurgeon to determine both the degree of its dilution, and the caſes in which it may be employed with advantage.

The dephlogiſticated marine acid, as is generally known, has the power of deſtroying the colour, the ſmell, and perhaps the taſte, of the greater part of animal and vegetable ſubſtances. We have ſeen that it corrects the fetor of putrid fleſh. And I have found, that, when it is poured in ſufficient quantity upon hemlock and opium, theſe narcotics ſpeedily loſe their ſenſible qualities. As it appears, therefore, to poſſeſs the power of correcting the vegetable, and probably many of the animal poiſons, it ſeemed not unlikely, that it might be uſeful as an internal medicine. Conceiving that its exhibition

would be perfectly ſafe, I once took twenty drops of it diluted with water. I ſoon afterwards, however, felt an obtuſe pain, with a ſenſe of conſtriction, in my ſtomach and bowels. This uneaſineſs, notwithſtanding the uſe of emetics and laxatives, laſted for ſeveral days, and was at length removed by drinking water impregnated with ſulphureous hepatic air. I afterwards found, that the manganeſe, which had been uſed in the diſtillation of the acid, contained a ſmall portion of lead.

Dr. INGEN-HOUSZ informed me, that a Dutchman of his acquaintance, ſome time ago, drank a conſiderable quantity of the dephlogiſticated marine acid: the effects which it produced were ſo extremely violent, that he narrowly eſcaped with his life. If therefore this acid ſhould hereafter be employed as an internal medicine, it would be neceſſary to prepare it by means of manganeſe that has been previouſly ſeparated, by a chemical proceſs, from the lead and the other metals with which that ſubſtance is uſually contaminated.

XXIII. *On the Satellites of the Planet Saturn, and the Rotation of its Ring on an Axis.* By William Herſchel, *LL.D. F. R.* S.

Read June 17, 1790.

IN my laſt Paper on the Planet Saturn, the principal object of which was to give an immediate account of the moſt intereſting phænomena that had occurred till the beginning of November, many things were left unnoticed for want of time to treat of them with ſufficient accuracy; but having now before me the whole ſeries of obſervations from the 18th of July till the 25th of December, 1789, I can enter into a proper examination, aſſiſted by ſuch neceſſary calculations as then could not conveniently be made.

One of the principal motives which have induced me to haſten this inquiry, is the frequent appearance of protuberant and lucid points on the arms of the ring of Saturn. I have mentioned before that ſuch phænomena had been reſolved by the ſituation of ſatellites that put on theſe appearances; but as my obſervations were continued near two months afterwards, and as I had from them corrected the epochæ of the old ſatellites, and improved the tables of the new ones, I found that, beſides many of theſe bright points which were completely accounted for by the calculated places of the ſatellites, there were alſo many more mentioned in my journal that would not accord with the ſituation of any of them.

The queſtion then preſented itſelf very naturally, what to make of theſe protuberant points? To admit two or three more ſatellites by way of ſolving ſuch phænomena appeared to me too hazardous an hypotheſis; eſpecially as theſe lucid points, though ſome of them had a motion, did not ſeem willing to conform to the criterion I had before uſed of coming off the ring, and ſhewing themſelves as ſatellites. And yet a ſuſpicion of at leaſt one more ſatellite would often return; it was even conſiderably ſtrengthened when I diſcovered, by means of re-calculating with great preciſion the whole ſeries of obſervations, that in the beginning of the ſeaſon there had been ſome few miſtakes in the names of the ſatellites, when the obſervations of them were entered in the journal. In ſetting them right, which threw a great light upon the revolution of the 6th, and more eſpecially upon that of the 7th, I found alſo, that ſome of the obſervations which were entered by the name of the 7th ſatellite could not belong to that, nor to any other known one. It remained therefore to be examined whether there might not be ſufficient ground to ſuſpect the exiſtence of an eighth ſatellite.

In this ſituation of things, I thought it moſt adviſable to draw out the whole ſeries of obſervations in a paper, beginning at the 5th ſatellite, and thus gradually through the 4th, 3d, 2d, 1ſt, 6th, and 7th, to approach towards the center of Saturn; that it might appear at laſt what obſervations were left unaccounted for. By this means alſo it will be ſeen clearly with how ſcrupulous an attention the identity of every ſatellite has been aſcertained; and with a view to give the ſtrongeſt ſatisfaction in this reſpect, at leaſt one obſervation of each has been calculated for each night; and the place thus computed is

put

put down in the notes, that it may be compared with the obſerved one.

To facilitate this compariſon, I have delineated a ſcheme *, wherein the orbits of the ſatellites are drawn in their due proportion. A few words will explain the conſtruction and uſe of this figure, which, notwithſtanding its ſimplicity, is yet amply ſufficient to aſcertain the accuracy of every obſervation.

In each of the orbits, by way of marking them, is placed the ſatellite to which it belongs, as it appeared to be ſituated the 18th of October, 1789. The graduated circle is of uſe to find, by means of the tables, the apparent place of a ſatellite for any given time; or, the apparent ſituation of the ſame ſatellite being given, its real Saturnicentric place may be deduced from it. In the center of the ſcheme is the planet Saturn, and its ring, expreſſed by a line which repreſents the direction of its anſæ; or the ring itſelf, as it appeared in my teleſcopes during the months of July, Auguſt, September, October, and November, 1789. The five lines which are carried on parallel to each other ſerve to convey the meaſure of the planet, and its ring, to the orbits of the ſatellites, as will be ſeen in ſeveral inſtances that occur hereafter.

The graduated circle is divided into degrees, and begins to count from that part of every ſatellite's orbit beyond the planet, which is intercepted by a plane paſſing from the eye of the obſerver, at rectangles to the ring, through the center of Saturn. Hence it follows, that the point of zero, or 360 degrees, is the ſame with the geocentric place of the planet in thoſe four parts of the orbit of the ſatellite where the eye is in the plane of the ring, and where it appears the moſt open; and that, in other places, it may be had by ſolving one ſpherical triangle. This is to be underſtood as relating only to

* See Tab. XIX. fig. 1.

the

the inner ſatellites; the 5th, or outermoſt, requiring a different reduction, on account of its deviation from the plane of the ring. Moreover, I am inclined to believe, that the ſureſt way of obſerving the 5th, is to truſt only to meaſures, taken with micrometers which give the diſtance and angle of poſition, except in ſuch caſes when the eye is nearly in the plane of this ſatellite's orbit, where the different reductions may be neglected, without bringing on any conſiderable inaccuracies. The order of the numbers by which 90 comes to the left, and 270 to the right, is taken from the motion of the ſatellites, as they appear to revolve in their orbits, when ſeen in teleſcopes of my conſtruction; and which is alſo the real direction of their motion according to the order of the ſigns. But the points 360 and 180 muſt occaſionally be changed in their denomination of north or ſouth, according to the real ſituation of the plane of each ſatellite's orbit. At preſent, for inſtance, when the ſatellites are at 360, that part of their orbits in which we find them lies to the ſouth of the center of the planet; but about the end of Auguſt, 1789, and afterwards, the orbits of the ſix inner ſatellites were differently ſituated; ſo that the ſame points then were turned towards the north. I need not remark, that the ſituation of theſe points was changed again when the earth paſſed through the plane of the ring, and that it will change, in the 5th ſatellite likewiſe, when we come to be in the plane of its orbit.

The calculations of the places of all the ſatellites have been made according to tables which are given at the end of this Paper. Their form being very ſimple, I thought it not amiſs to communicate them, for the uſe of thoſe who may wiſh to enter into a more particular examination of the following obſervations; or to follow the ſatellites in their orbits at any

future time. It will be proper to mention, that I have deduced the epochæ of all the ſeven ſatellites from my own obſervations, and they will be found to differ conſiderably from thoſe which are given by M. DE LA LANDE, in the *Connoiſſance des Temps* for 1791. But I have not attempted to extend them farther than a few years backwards or forwards, as I am not in poſſeſſion of any obſervations that could authorize me to undertake ſuch a work. On the contrary, I am well convinced, that no tables will give us the ſituation of the ſatellites accurately, till we have at leaſt eſtabliſhed the dimenſions of their elliptical orbits, and the motion as well as the ſituation of their aphelia. The epochæ for 1789, therefore, muſt be looked upon not as *mean* ones, but ſuch as reſpect the orbits of theſe ſatellites in their ſituation during the time of the following obſervations; and the two preceding, and two following years, muſt be already a little affected with thoſe errors which are the neceſſary conſequence of our not knowing the required elements. I flatter myſelf, however, that the obſervations, which are delivered in this Paper, will ſerve as a beginning to a proper foundation for inveſtigating them. The many conjunctions between the ſatellites, for inſtance, will undoubtedly throw ſome light on the ſituation and excentricity of their orbits; as it will be found, that the calculated places of theſe conjunctions require elliptical motions to bring the ſatellites to ſuch appearances, which, in circular orbits, could not ſo accurately have taken place. Nor can we aſcribe the diſagreements to the fault of the obſervations, ſince a very few minutes will ſuffice to determine the time of a conjunction, which never laſts long. For this reaſon alſo, I have carefully avoided deducing my epochæ from conjunctions, even with the 6th

ſatellite,

ſatellite, which moves ſo rapidly that, at firſt ſight, we might think thoſe ſituations favourable.

The mean motion of the five old ſatellites, as being ſufficiently accurate for my preſent purpoſe, I have taken from the above-mentioned tables of M. DE LA LANDE; and thoſe of the 6th and 7th, of courſe, are the reſult of my own obſervations.

The geocentric place of Saturn, whoſe complement is to be added, in order to reduce the Saturnicentric ſituation of the ſatellites to the apparent one, I have taken from the nautical almanac to the neareſt minute; and, as I have always confined myſelf to a literal tranſcription of the obſervations from the original journal, all the memorandums which are neceſſary either to explain them, or to correct miſtakes in the names of the ſatellites, are thrown into notes, that there may be no interruption in the ſucceſſion of the obſervations.

Obſervations on the fifth ſatellite of Saturn.

1789, July 18. 20h. 20′ (A). The ſuppoſed fifth ſatellite (B) 6° or 7° ſp. (C) the ring (D).

July

(A) The time of my obſervations being ſidereal, it is neceſſary to mention, that this relates only to the hours, minutes, and ſeconds, the day itſelf being that which is generally uſed by aſtronomers, beginning at noon, and ending the noon following. By this means there can never be a miſtake which ſidereal hour I mean to point out, as no two ſuch hours can occur in the ſame aſtronomical night.

It will alſo be neceſſary to remark, that all the times are thoſe ſhewn by the clock; which, by equal altitudes, has been found to loſe very equally at the rate of 0″,4 *per* day; and to be 8′ 51″,5 too faſt at midnight the 18th of July, 1789, which is the time on which my obſervations on Saturn commenced.

(B) The ſatellite itſelf not being known, it is here called the ſuppoſed fifth.

(C) By ſix or ſeven degrees ſouth preceding the line of the ring, is meant, that

July 23. 19 29. The 5th ſp. at a great diſtance (E).

July 28. 22 37. By a figure in the journal, at a great diſtance np. (F).

Aug. 18. 21 11. The ſuppoſed 5th at a great diſtance 25° np. R. (G).

Aug. 28. 1 28. A line drawn through a large ſtar north of Saturn, and paſſing between one pretty conſiderable ſtar nf. and another ſf. Saturn, leaves the ſuppoſed 5th ſatellite a little on the following ſide. By two figures in the journal, the 5th is at a great diſtance nf. ♄. (H).

Aug. 29. 23 29. The ſuppoſed 5th is a very little preceding a line drawn from the large ſtar of laſt night, through a very ſmall ſtar; and a good deal following a line drawn from the

that the ſatellite in the firſt place was at the preceding ſide of the planet; that is, in the ſemicircle from 180 to 360, which paſſes through 270 degrees. And in the next, that the ſituation of its orbit was ſuch as to bring the ſatellite, at its proper diſtance, into a line drawn from the center of Saturn, making an angle of 6 or 7 degrees with the line of the ring, and declining towards the ſouth.

(D) The calculated place for 20 h. 20′, ſhewn by the clock, corrected by −8′ 51″,5, and reduced to 12 h. 22′ 16″ mean time, is 245°,5 which, as no diſtance is mentioned, leaves it doubtful whether the obſervation was that of the 5th ſatellite, or of a fixed ſtar.

(E) By calculation the ſituation is 268°,1; which agrees well enough for 11 h. 11′ 47″ mean time.

(F) 13 h. 59′ 38″ mean time gives 291°,5, which agrees with the diſtance and direction; but as the ſatellite was ſp. the obſervation, which ſays np. muſt belong to ſome ſmall fixed ſtar.

(G) The calculation for 11 h. 11′ 27″ gives 27°,4 or at a good diſtance ſf; therefore this was not the ſatellite, but a ſtar.

(H) It appears from the calculation for 14 h. 48′ 29″ which gives 74°,2, and alſo from the following obſervations, that this was the real 5th ſatellite; and that, having once obtained its place, I kept it in view all the reſt of the ſeaſon.

firſt pretty conſiderable ſtar of laſt night, through the ſame very ſmall ſtar (I).

Aug. 31. 21 3. The 5th I take to be nf. ½ at a good diſtance (K).

Sept. 8. 22 37. The 5th about 15° nf. R. and, by a figure, at a great diſtance (L).

Sept. 11. 20 11. The ſuppoſed 5th ſatellite and two ſmall ſtars ſf. a ſtar *x*, which is ſf. ½, form an exact line.

22 32. The ſuppoſed 5th, and the two S ſt. ſf. *x*, form no longer a line; ſo that is the real fifth ſatellite.

Sept. 11. 23 52. The 5th ſatellite keeps advancing; its ſituation is 20 or 22° nf. the line of the R. and, by a figure, it is at a conſiderable diſtance (M).

Sept. 13. 22 17. The 5th ſat. of the 11th of Sept. is advanced, and is now north preceding a conſiderable large ſtar, which was that night ſp. ½. By a figure it is nf. ½, at a conſiderable diſtance (N).

Sept 14. 20 33. The 5th a little nearer than laſt night (O).

22 30. The 5th ſat. of Sept. 11. obſerved at 20 h. 11′, has left the place where it was at that time.

Sept. 16. 19 39. The 5th is drawing nearer towards its conjunction.

22 18. Much the ſame as before.

23 59. About 33° north following the direction of the R. (P).

1 3. The 5th nearly as before.

	h.	′	″	°
(I)	12	45	53.	78,4.
(L)	11	14	47.	124,2.
(N)	10	35	13.	147,2.
(P)	12	5	9.	161,3.

	h.	′	″	°
(K)	10	12	26.	87,2.
(M)	12	17	48.	138,3.
(O)	8	47	34.	151,5.

Sept.

Sept. 17. 19 48. The 5th ſat. of ♄ 30° nf. R. and at the diſtance of about 3 dia. of R. (Q).

Sept. 18. 21 15. About 2 dia. of R. and near 40° north following (R).

Sept. 20. 23 24. The 5th ſat. is within a degree of its conjunction. It is north of ♄, and its motion is retrograde.

23 54. A perpendicular from the 5th ſat. to the ring of Saturn, falls towards the following ſide ſhort of the center by $\frac{1}{6}$ dia. of ♄.

0 19. Diſtance of the 5th ſatellite from the parallel of the R. of ♄, 3 rev. 36,7 parts = 1′ 0″,966 central meaſure.

1 25. The 5th very nearly central.

1 28. With a power of 240, perfectly central. With 300, perfectly central (S).

Sept. 21. 21 15. The 5th ſat. is perpendicular to a place half a projection of the ring preceding the edge of it (T).

Sept. 23. 22 51. At a conſiderable diſtance np. ♄ (V).

Sept. 24. 19 56. At a good diſtance np. ♄ (W).

Sept. 25. 19 34. The 5th purſues its track (X).

	h.	′	″	°		h.	′	″	°
(Q)	7	50	55.	165,1.	(R)	9	13	45.	170,0.
(S)	13	18	13.	180,0, or directly at rectangles to the ring, to the north.					

I have uſed this obſervation for ſettling the epocha of this ſatellite, in which I have made no other allowance than that of the geocentric place of Saturn, as I knew this would anſwer all my purpoſes. But when we would obtain the mean motion of this ſatellite in comparing its preſent place with other ſituations at a great diſtance of time, proper reductions of the geocentric place of ♄ to the orbit of this ſatellite ſhould be made. This may, however, be done much better when the real ſituation of its orbit is properly aſcertained.

	h.	′	″	°		h.	′	″	°
(T)	9	1	59.	183,8.	(V)	10	29	52.	193,3.
(W)	7	31	25.	197,4.	(X)	7	5	33.	201,9.

Oct. 12. 21 18. The ſuppoſed 5th forms nearly an iſoſceles triangle with two preceding ſtars, the ſouthern one of which is double, conſiſting of a very conſiderable ſtar and a ſmall one. By a figure, at a conſiderable diſtance, np. ♄ (Y).

Oct. 15. 21 1. The large ſtar of the double ſtar in the figure of the 12th of Oct. is gone from its place, and the ſuppoſed 5th of that night is left (Z).

21 8. The real 5th is ſo bright this evening, and was ſo the 12th of Oct. that I miſtook it on that account for a conſiderable ſtar; it was then nf. ♄. By three figures to-night it is at a great diſtance np. ♄. I ſaw it move to-night; for at 21 h. 1′ it made an angle of 50° on the following ſide with three ſtars in a line, ſp. ♄. At 1 h. 9′, that angle was leſs than 40°; and at 1 h. 41′, it was no more than about 35°.

Oct. 16. 20 16. The 5th now precedes a line drawn through the three ſtars which it followed laſt night at 21 h. 1′. By five figures, at a great diſtance ſp. ♄ (A).

Oct. 18. 20 18. At a great diſtance ſp.

21 51. At 7 or 8′ diſtance ſp. The ſame by two figures (B).

Oct. 20. 20 50. By three figures, at a great diſtance ſp. (C).

Oct. 28. 21 1. The 5th about $3\frac{1}{2}$ dia. of ♄ diſtant, and 45° ſp. (D).

Oct. 29. 21 49. The 5th ſat. of ♄ is approaching towards its oppoſition (E).

(Y) 7 h. 42′ 32″. 280°,3. The diſtance and ſituation agree well enough, but not the angle, which, by what will appear hereafter from the ſituation of the nodes of this ſatellite, ſhould be ſp.

	h.	′	″	°		h.	′	″	°
(Z)	7	13	49.	294,0.	(A)	6	25	0.	298,4.
(B)	7	51	54.	307,9.	(C)	6	43	13.	316,8.
(D)	6	22	47.	353,4.	(E)	7	6	44.	358,1.

Oct.

Oct. 30. 20 53. The 5th ſat. is paſt the oppoſition a little more than yeſterday it wanted of it (F).

Oct. 31. 21 13. The 5th about $3\frac{1}{2}$ diameters of Saturn ff. (G).

21 43. It is very faint; fainter than the firſt; not much brighter than the ſixth (H).

Nov. 2. 23 57. By a figure, the 5th ſat. at ſome diſtance ff. (I).

Nov. 3. 22 1. At a good diſtance ff. R. (K).

22 13. The 5th a little following a line drawn through two fixed ſtars and between them. It is ſouth of the ring.

Nov. 4. 22 17. The 5th at a great diſtance following, and a very little ſouth; it precedes the line of the two ſtars which it followed laſt night (L).

Nov. 7. 22 9. The 5th at a great diſtance following, and a little north (M).

Nov. 8. 20 46. At a great diſtance following (N).

Nov. 10. 23 30. As the calculation gives it (O).

(F) 6 h. 6′ 57″. 2°,5. (G) 6 h. 22′ 59″. 7°,1.

(H) From the conſiderable change in the light of this ſatellite, we may ſurmiſe, that it has a revolution upon its axis; the ſituation (ſee note Z and G), which affects the apparent brightneſs, ſhould however be taken into the account.

(I) 8 h. 58′ 41″. 16°,8. (K) 6 h. 59′ 4″. 21°,0.

(L) 7 h. 11′ 6″. 25°,2. A few days ago I perceived, that in the former part of theſe obſervations I had omitted a pretty eſſential circumſtance, which is an attention to the nodes of the 5th ſat. with the ring of Saturn.

(M) 6 h. 51′ 21″. 39°,2. It appears from this and the foregoing obſervation, that the aſcending node of the 5th ſatellite, with regard to the ring of Saturn, apparently lies between the 25th and 39th degrees, which, reduced to a Saturnicentric poſition, is about the 19th degree from the point Aries reckoned upon the ring.

(N) 5 h. 24′ 39″. 43°,5. (O) 8 h. 0′ 21″. 53°,0.

Nov. 13. 22 33. On the following ſide (P).

Nov. 15. 22 33. By a figure, nf. Saturn at a great diſtance (Q).

Nov. 19. 22 15. Diſt. of the 5th. ſat. 1ſt meaſure 8 54,94
but too ſmall.

22 28. — — 2d — 8 58,28
22 43. — — 3d — 8 58,23
22 54. — — 4th — 8 58,85

Mean of the three laſt meaſures 8′ 58″,45.

This, when the exact inclination of the orbit is aſcertained, muſt be brought to the greateſt elongation, and alſo reduced to the mean diſtance of the planet from the ſun.

Dec. 2. 0 56. The 5th ſat. is in its calculated place (R).

Dec. 5. 0 10. As the calculation gives it (S).

Dec. 16. 23 59. At a great diſtance preceding (T).

Obſervations on the fourth ſatellite of Saturn.

July 18. 19 50. The 4th ſatellite is about 6 or 7° np. R. (A).

July 23. 19 29. About 3½ dia. of ½ following the body (B.

	h.	′	″	°		h.	′	″	°
(P)	6	51	44.	66,5.	(Q)	6	43	53.	75,6.
(R)	7	59	46.	152,8.	(S)	7	2	7,	166,2.
(T)	6	7	58.	215,4.	(A)	11	52	21.	284,0.
(B)	11	11	47.	36,4.					

Following or preceding the *body*, denotes that we are to reckon from the neareſt part of the circumference, and not from the center; but it is alſo to be obſerved, that eſtimations in diameters, when they exceed one, or one and an half, are not intended as meaſures, but merely to point out the ſituation in a very coarſe way; ſo that we are to look upon the calculation as not diſagreeing with this eſtimation, though we ſhould find the ſatellite conſiderably farther from the body.

July 27. 20 27. 4 or 5 dia. following (C).

July 28. 19 40. Near 4 dia. following (D).

Aug. 18. 21 11. Many dia. preceding (E).

Aug. 28. 0 14. About $4\frac{1}{2}$ dia. of ♄ following the body (F).

Aug. 29. 22 18. 3 dia. of ♄ following the body (G).

Aug. 31. 20 48. About $2\frac{1}{4}$ dia. of ♄ p. the body; a few ſeconds farther off than the 2d ſatellite, and a little ſouth of it (H).

Sept. 8. 22 30. About $2\frac{1}{2}$ dia. of ♄ following the body (I).

Sept. 10. 19 42. Following Saturn (K).

Sept. 11. 20 26. Following ♄, too far to eſtimate by the diameter (L).

Sept. 13. 22 17. By a figure, at a diſtance following (M).

Sept. 14. 20 33. About $1\frac{1}{4}$ dia. of the ring following (N).

0 42. $1\frac{1}{4}$ dia. of R. f. the edge.

1 24. 1 dia. of R. f. the ring; exactly in the line.

Sept. 16. 19 37. Not quite 1 dia. of the ring preceding (O).

22 15. $1\frac{1}{2}$ dia. of R. p. the edge, and a little ſouth.

23 59. Near $1\frac{1}{2}$ dia. of R. preceding the edge of the ring.

Sept. 17. 19 48. About 3 dia. of the ring p. the projection (P).

Sept. 18. 21 15. Almoſt at its greateſt diſtance p. (Q).

	h.	′	″	°
(C)	11	53	55.	127,5.
(D)	11	3	7.	149,3.
(E)	11	11	27.	264,7.
(F)	13	34	42.	133,4.
(G)	11	35	5.	154,2.
(H)	9	57	29.	198,0.
(I)	11	7	48.	20,3.
(K)	8	12	24.	62,8.
(L)	8	52	22.	86,1.
(M)	10	35	13.	133,0.
(N)	8	47	34.	154,0,
(O)	7	43	52.	198,3.
(P)	7	50	55.	221,1.
(Q)	9	13	45.	245,0.

Sept.

Sept. 20. 23 24. At a great diſtance p, and a little n. (R).

0 40. I can ſee the 4th ſatellite of Saturn without an eye-glaſs in the 20-feet ſpeculum by drawing back the eye about three or four feet.

21. Sept. 21 15. At a good diſtance preceding, and a little north (S).

Sept. 23. 22 33. The 4th ſatellite emerged a few ſeconds ago. It is now in the line of a tangent to that part of Saturn where the projection of the ring comes from the body (T).

22 51. $\frac{1}{3}$ of the projection (V) following the body of ♄; and about 2 of its own diameters north of the ring; or not quite half way northwards between the center of ♄ and the northern limb.

Sept. 24. 19 56. About 2 of its own dia. nearer the ring than the 3d ſat. and a little more north.

20 48. The 4th advances to its conjunction with the 3d.

22 47. The 4th is paſt by the 3d. By a figure, it is leſs than one of its diameters paſt the 3d ſatellite, and is more north than the 3d (W).

Sept. 19 34. It purſues its track (X).

Oct. 12. 20 37. At a great diſtance following (Y).

Oct. 15. 20 54. Many diameters of Saturn f. (Z).

23 20. The 4th ſat. at a conſiderable diſt. f.

	h	′	″	°
(R)	11	14	33.	292,.2
(T)	10	11	55.	359,2.

	h.	′	″	°
(S)	9	1	59.	312,8.

(V) The diſtance from the body of Saturn to the end of the projecting part of the ring, I call the projection, and have made uſe of it as a meaſure for eſtimating.

	h.	′	″	°
(W)	10	21	57.	22,0.
(Y)	7	1	39.	66,5.

	h.	′	″	°
(X)	7	5	33.	41,6.
(Z)	7	6	50.	134,4.

Oct.

Oct. 16. 20 16. 2½ dia. of ♄ following the body.

21 59. The colour of the 4th ſatellite is red, or inclining to red; it approaches towards a conjunction with the 3d.

0 9. The 4th is very nearly in conjunction with the 3d; it is about ¼ of its own dia. nearer to Saturn than the 3d, and near one dia. of the 3d ſatellite more ſouth than the 3d (A).

Oct. 18. 20 18. 2½ dia. of ♄ p. the body.

21 51. About 2½ dia. of ♄ preceding (B).

Oct. 20. 20 50. At a great diſtance preceding (C).

Oct. 28. 21 1. At a great diſtance following (D).

Oct. 29. 21 49. At a good diſtance ſf (E).

Oct. 30. 20 53. At a great diſtance following (F).

Oct. 31. 21 13. At a conſiderable diſtance following (G).

Nov. 2. 21 6. The 4th ſat. is inviſible.

21 51. I cannot ſee the 4th ſatellite (H).

22 53. The 4th ſat. is not viſible; I looked for the ſhadow of it upon ♄, but could not perceive it. The weather a little hazy.

23 21. Upon the dark equatorial belt of ♄, on the f. ſide, near the edge of the diſk, ſeems to be a ſmall black ſpot which is darker than the reſt of the belt. ✓

	h.	′	″	°
(A)	10	17	22.	160,1.
(B)	7	51	54.	203,0.
(C)	6	43	13.	247,2.
(D)	6	22	47.	67,9.
(E)	7	6	44.	91,2.
(F)	6	6	57.	112,8.
(G)	6	22	59.	135,7.

(H) The tables I uſed at the time of this obſervation being different from my preſent ones, I expected the 4th ſatellite to be paſt its conjunction, and conſequently viſible again.

23 29. A protuberance on the ſp. part of ♄; I ſuppoſe it to be the 4th ſatellite emerging.

23 31. The black ſpot upon the equatorial belt ſeems to be a little advanced towards the preceding ſide.

23 38. With 300 the ſatellite is very nearly detached; the black ſpot keeps advancing; it is a very little north of the equatorial belt, but part of it is upon the belt.

23 43. The black ſpot is a little more than $\frac{1}{8}$ of the dia. of ♄ advanced from the f. ſide towards the center.

23 46. The ſatellite ſeems to be detached.

23 47. With 300, it is detached; and the black ſpot keeps advancing.

23 57. The black ſpot is advanced ſo as to be $\frac{1}{3}$ of its way towards the center; the 4th ſatellite is near $\frac{1}{2}$ its own dia. clear of the edge.

0 13. The black ſpot a little more than half way towards the center; it is much darker than the belt.

0 34. The black ſpot is not arrived to the center yet.

0 53. The black ſpot is not come to the center, but does not want much of it.

0 57. It is more upon the belt than it was before; that is, more ſouth.

1 6. The black ſpot is not yet come to the center.

1 10. It is drawn towards the ſouth, ſo as to be nearly in the middle of the equatorial belt.

1 11. It is not far from a central poſition.

1 15. It is not come to the center yet.

1 18. The black ſpot is very near central.

1 21. Very near central.

1 25. Begins to be in the center.

1 30. It is in the center (I).
Nov. 3. 22 1. The 4th ſat. about 3 dia. p. the body (K).
Nov. 4. 22 17. At a conſiderable diſtance preceding (L).
Nov. 7. 22 9. At a good diſtance preceding (M).
Nov. 8. 20 46. At a good diſtance p. (N).
Nov. 10. 21 3. The 4th ſatellite not yet viſible.
21 32. Not yet viſible.
22 26. Not yet viſible.
23 24. About ½ dia. of ♄ nf. (O).
Nov. 13. 22 33. On the following ſide (P).
Nov. 15. 22 33. Following ♄ at a good diſtance (Q).
Nov. 26. 0 28. The 4th ſatellite is emerged ſome time paſt.
0 30. It is nearly in conjunction with the 6th. By a figure, it is half the diameter of the 6th nearer to Saturn than the ſixth, and north of it (R).

Nov. 30. 23 36.	Diſt. of the 4th ſatellite			3′ 12″.379.
23 42.	2d meaſure	—		3 10 .972.
23 52.	3d	—	—	3 10 .494.
23 59.	4th	—	—	3 10 .579.

Mean of the four meaſures 3′ 11″.106 (S).

Dec.

(I) 10 h. 31′ 25″,5. 184°,8. An extract of theſe obſervations being printed in my laſt Paper, I am to remark, that here the time is uncorrected; but the correction for this evening being —8′ 8″,7, it will be ſeen, that in the former Paper —8′ has been applied to all the times, and —8′ 9″ to the time of the exact conjunction.

	h.	′	″	°		h.	′	″	°
(K)	6	59	4.	204,1.	(L)	7	11	6.	226,9.
(M)	6	51	21.	294,4.	(N)	5	24	39.	315,6.
(O)	7	54	22.	3,1.	(P)	6	51	44.	69,9.
(Q)	6	43	53.	115,0.	(R)	7	57	23.	4,4.

(S) The middle of the time to which we may ſuppoſe the meaſures to anſwer

Dec. 2. 0 56. In its calculated place (T).

Dec. 5. 0 10. As the calculation gives it (V).

Dec. 16. 23 59. At a great diſtance following (W).

0 43. The 4th ſatellite, with a power of about 500, ſhews a pretty conſiderable, viſible diſk (X).

Obſervations on the third ſatellite of Saturn.

July 18. 19 50. The 3d ſatellite about 1 or 2° ff. R. By a figure, at a conſiderable diſtance (A).

July 23. 19 29. Near 2 dia. of ♄ following (B).

July 27. 20 27. About 2½ dia. of ♄ following (C).

July 28. 19 40. The 3d ſat. ½ dia. following ♄; it is much larger than the 2d, and a little more north.

22 34. ⅓ part of a dia. following (D).

Aug. 18. 21 11. 1½ dia. of ♄ following the ring (E).

Aug. 28. 0 14. Full 2 dia. f. ♄ (F).

is 23h. 48′, or 6 h. 59′ 48″ mean time. And by computation the apparent place of the ſatellite at that time was 93°,778, which is 3°,778 or 3° 46′ 41″,52 paſt the greateſt elongation; therefore its diſtance, if it had been meaſured at the greateſt elongation, would have been 3′ 11″,522. This quantity brought to the mean diſtance of Saturn from the ſun, amounts to 3′ 8″,918.

	h.	′	″	°
(T)	7	59	46.	139,8.
(V)	7	2	7.	206,6.
(W)	6	7	58.	93,6.

(X) And from its ruddy colour (ſee Oct. 16.) we may ſurmiſe it to have a conſiderable atmoſphere. This ſatellite, therefore, ſeems to approach more to the condition of a planet than any of the fourteen known ſatellites.

	h.	′	″	°
(A)	11	52	41.	85,5.
(B)	11	11	47.	121,9.
(C)	11	53	55.	83,1.
(D)	13	56	39.	169,6.
(E)	11	11	27.	35,0.
(F)	13	34	42.	120,6.

Aug. 29. 22 21. A ſatellite on the edge of the preceding arm (G).

23 1. The ſat. a very little ſeparated from it. I ſuppoſe it to be the 3d, on account of its ſize and brightneſs.

23 41. The ſatellite is now fully detached, ſo as to be near $\frac{1}{3}$ of the projection preceding the end of it.

Aug. 31. 20 56. The preceding arm, about the middle, ſeems to be charged with a ſatellite; power 157 (H).

21 3. With 300, the ſame as before.

Sept. 8. 22 30. The 3d ſat. about $2\frac{1}{2}$ or 3 dia. of Saturn p. the body (I).

Sept. 10. 19 42. The 3d following Saturn (K).

Sept. 11. 20 26. $1\frac{1}{2}$ dia. of ♄ f. the body of ♄.

22 36. 1 dia. of ♄ f. the body.

0 16. The 3d a little leſs than the projection from the f. edge.

0 34. $\frac{3}{4}$ of the projection following the ring.

1 57. A little leſs than $\frac{1}{2}$ the projection f. the ring (L).

Sept. 13. 22 2. The 3d, $1\frac{1}{4}$ dia. of R. preceding the edge of R. (M).

Sept. 14. 20 27. The 3d ſat. $\frac{1}{2}$ the projection f. R. (N).

21 55. $1\frac{1}{3}$ of the projection f. and a very little north.

0 42. 2 projections following.

1 24 Near two projections following.

Sept. 16. 22 18. (O).

	h.	′	″	°
(G)	11	38	5.	193,9.
(I)	11	7	48.	269,9.
(L)	14	22	27.	159,9.
(N)	8	41	35.	20,4.
(O)	10	24	26.	185,6.

	h.	′	″	°
(H)	10	5	27.	348,3.
(K)	8	12	24.	59,7.
(M)	10	20	15.	306,1.

By this it appears that the 3d ſatellite was inviſible; but obſervations being made on a ſatellite, by miſtake ſuppoſed to be the third, they will be found among thoſe of the 6th, to which they belong.

Sept.

Sept. 17. 19 48. The 3d ſat. 1¾ dia. of the ring preceding the projection (P).

Sept. 18. 21 15. ⅓ of the projection, or 1½ dia. of the ſatellite preceding the edge of the ring.

21 45. ¼ of the projection preceding R.

21 53. The 3d almoſt touches the R.

21 59. Quite cloſe to the ring, and a little north.

22. 7. Not quite ſo near but that I can ſtill ſee a ſmall diviſion.

22 20. With 157, I can no longer ſee a diviſion between the 3d ſatellite and the R.

22 22. With 300, the ſat. is completely joined to the R. but ſo as to make it appear a little longer, and a very little knotty towards the north (Q).

Sept. 20. 23 27. The 3d ſat. 1½ projection f. R. It is within leſs than the diameter of the 2d ſatellite preceding the 2d, and a little more ſouth (R).

23 51. The 3d ſat. is now more ſeparated from the 2d.

0 45. 1¼ projection f. R.

1 22. 1 projection f. R.

Sept. 21. 21 15. 2½ projections preceding the edge.

22 44. Near 3 projections p. R. (S).

Sept. 23. 22 51. The 3d ſat. 1 projection f. the edge (T).

Sept. 24. 19 56. 1½ dia. of R. f. the edge; about 2 diameters of the 4th ſatellite farther from R. than the 4th, and a little more ſouth.

20 48. It advances towards a conjunction with the 4th.

	h.	′	″	°
(P)	7	50	55.	256,9.
(R)	11	17	33.	147,6.
(T)	10	29	52.	24,3.

	h.	′	″	°
(Q)	10	20	34.	345,0.
(S)	10	30	44.	224,8.

22 47.

22 47. It is paſt by the 4th. By a figure, it is about half its own diameter paſt the conjunction, and is more ſouth than the 4th (V).

Sept. 25. 19 34. (W).

Oct. 12. 20 37. The 3d ſat. about $3\frac{1}{2}$ dia. of ♄ f. the body (X).

Oct. 15. 20 47. $1\frac{1}{4}$ dia. of ♄ p. the body.

21 30. A little more than 1 dia. of ♄ from the body; and its whole diameter north of the line of the projection.

22 25. The 3d ſat. will be in conjunction with the 6th, in a very ſhort time, the 3d being ſtill a little preceding.

22 39. The conjunction is ſo complete now, that I have loſt the 6th. The 3d, however, appears to be a little lengthened out towards the ſouth. Diſtance from the body barely one diameter of ♄; or juſt one dia. of ♄ including the dia. of the 3d (Y).

23 54. Near two of its own diameters paſt the conjunction with the 6th.

0 59. The 1ſt, the 3d, and the 6th, are at equal diſtances from each other.

1 14. The 3d is nearer to the 1ſt than to the 6th.

1 35. The 3d approaches to a conjunction with the 1ſt.

1 45. The 3d is very near its conjunction with the 1ſt. By a figure, it wants leſs than $\frac{1}{2}$ a dia. of the 3d.

Oct. 16. 20 16. The 3d, $1\frac{3}{4}$ dia. of ♄ following the body.

21 59. It draws towards a conjunction with the 4th. The colour of the 3d is inclining to blue.

(V) 10 h. 21′ 57″. 103°,6.

(W) 7 h. 5′ 33″. 172°,5. Hence it appears that the ſatellite could not be ſeen this night.

(X) 7 h. 1′ 39″. 88°,2. (Y) 8 h. 51′ 33″. 333°,5.

0 9. The 3d is in conjunction with the 4th, or $\frac{1}{4}$ of the dia. of the 4th ſatellite paſt the conjunction; and one of its own dia. more north than the 4th; that is, there is a vacancy between them of one dia. of the 3d (Z).

Oct. 18. 20 18. 1 dia. of ♄ preceding the body.

21 51. $1\frac{1}{4}$ dia. of ♄ p. the body (A).

Oct. 20. 22 19. With 300, I ſee the 3d ſat. emerging; about $\frac{3}{4}$ of its own dia. is out, at the following ſide of ♄.

22 44. At the diſtance of $\frac{3}{4}$ of its dia. following the body (B).

0 8. The f. projection paſſes over the 3d ſat. juſt ſo as to clear it.

Oct. 28. 21 1. The 3d about 3 dia. of ♄ p. the body (C).

Oct. 30. 20 53. About 3 dia. of ♄ following the body (D).

Oct. 31. 21 13. $\frac{5}{8}$ dia. of ♄ following the body.

21 57. There is a complete conjunction between the 3d ſat. and the 2d; the following arm of the ring paſſes exactly between them, and points to the 6th. The diſtance between the 3d and 2d is about $\frac{1}{2}$ the diameter of the 3d ſatellite, the 2d being to the north, and the 3d to the ſouth (E).

22 0. I can ſee that the conjunction between the 3d and 2d ſatellites is paſt.

23 13. The 3d, $\frac{1}{4}$ dia. of ♄ following the body (F).

23 49. The 3d approaching to a contact with the body, but I can ſee a diviſion yet.

23 58. A diviſion ſtill viſible between the 3d ſatellite and the body of ♄.

	h.	′	″	¯ °
(Z)	10	17	22.	57,9.
(B)	8	36	54.	11,4.
(D)	6	6	57.	80,4.
(F)	8	22	39.	167,7.

	h.	′	″	¯ °
(A)	7	51	54.	209,4.
(C)	6	22	47.	281,8.
(E)	7	6	51.	163,5.

Nov. 2. 21 6. The 3d ſatellite, $1\frac{3}{4}$ dia. of ♄ p. the body; juſt following the 2d, but a little more north (G).

21 44. $1\frac{3}{4}$ dia. of ♄ preceding the body.

Nov. 3. 22 1. $2\frac{1}{4}$ dia. of ♄ f. the body (H).

Nov. 4. 22 17. About 2 dia. of ♄ following the body (I).

Nov. 7. From 21 h. 28′ to 23 h. 12′ (K).

Nov. 8. 20 46. At a conſiderable diſtance following (L).

Nov. 9. 1 2. $\frac{1}{8}$ dia. of ♄ following the body; its whole dia. is ſouth of the arm (M).

Nov. 10. 23 30. As the calculation gives it (N).

Nov. 13. 22 33. On the following ſide of ♄ (O).

Nov. 15. 22 33. At ſome diſtance, preceding (P).

Nov. 16. 22 50. (Q).

Nov. 21. 1 54. The 3d, about $1\frac{1}{2}$ dia. of ♄ f. the body (R).

Nov. 25. 1 21. (S).

Nov. 30. 23 47. The 3d ſat. about $2\frac{1}{2}$ of its own dia. following the 6th (T).

Dec. 2. 23 36. $\frac{3}{4}$ of the projection p. the body; its whole diameter is to the ſouth of the arm.

0 22. The 3d, the 1ſt, and the 6th, nearly at equal diſtances from each other.

	h.	′	″	°		h.	′	″	°
(G)	6	8	9.	319,6.	(H)	6	59	4.	42,5.
(I)	7	11	6.	122,5.					

(K) From 6 h. 10′ 28″ to 7 h. 54′ 11″. 358°,3 to 4°,0, and conſequently inviſible.

	h.	′	″	°		h.	′	″	°
(L)	5	24	39.	75,5.	(M)	9	36	1.	169,1.
(N)	8	0	21.	243,5.	(O)	6	51	44.	118,8.
(P)	6	43	53.	277,8.	(Q)	6	56	55.	358,2. inviſible.
(R)	8	40	57.	42,4.	(S)	8	52	10.	1,8. therefore inviſible.
(T)	6	58	48.	33,9.					

0 50. The 3d and 1ſt are in conjunction with a little ſpace between them; the 3d being to the ſouth, and the 1ſt to the north (V).

Dec. 5. 0 10. As the calculation gives it (W).

Dec. 16. 23 59. 1½ dia. of ♄ p. the body (X).

Obſervations on the ſecond ſatellite of Saturn.

July 18. 19 50. The 2d ſatellite in the line of the ring p. Saturn; but about 2 or 3° north (A).

July 23. 19 29. ¾ dia. of ♄ preceding (B).

July 27. 20 24. Upon the ſf. part of the R. are two ſmall bright points, the largeſt is to the ſouth, and is neareſt to the body of ♄. (C).

20 29. The largeſt of the knobs is about ¼ dia. of ♄ from the body. Memorandum, I have no doubt, but that the large knob is the 2d ſatellite; I could nearly ſee its whole diameter to the ſouth of the ring, but not ſeparated. Clouds came on (D).

July 28. 19 40. The 2d ſat. of ♄ ¾ dia. following.

22 34. Almoſt a dia. f. (E).

	h.	′	″	°
(V)	7	53′	47″.	196,3.
(X)	6	7	58.	225,5.
(B)	11	11	47.	205,8.

	h.	′	″	°
(W)	7	2′	7″.	72.4.
(A)	11	52	21.	271,6.

(C) By calculation we find that theſe two bright points were the 2d and 6th ſatellites; but at the time of theſe obſervations I only took down phænomena as they preſented themſelves, leaving a ſolution of them to future conſiderations. See Note (B) to the 6th ſatellite.

(D) 11 h. 55′ 55″. 16°,0. (E) 13 h. 56′ 39″. 158°,7.

Aug.

Aug. 18. 21 11. ¼ dia. f. R. (F).

Aug. 28. 0 14. Near 2 dia. of ♄ p. the body (G).

Aug. 29. 22 18. About 1 dia. of ♄ p. the body (H).

Aug. 31. 20 48. A few ſeconds nearear to ♄ than the 4th ſatellite, and a little more north (I)

Sept. 8. 22 30. The 2d ſat. 1¾ dia. of ♄ p. the body (K).

Sept. 10. 19 42. The 2d within 1 or 2 of its own diameters of the edge of the projection (L).

23 2. Inviſible.

Sept. 11. 20 26. The 2d, 1¾ dia. of ♄ preceding the body.

22 36. 1¼, or almoſt 1½ dia. of ♄ preceding the body.

1 34. 1¼ of the projection preceding R.

1 57. 1 projection p. R. (M)

Sept. 13. 22 6. There are two ſatellites emerging inſtead of one (N).

22 13. With 300, the neareſt, 1½ of its own diameters preceding the projection. This I take to be the 2d ſatellite (O).

Sept. 14. 20 27. Barely 1 projection p. R. and a little north.

21 55. The 2d, 1 dia. of the ſatellite p. R. (P).

22 23. The 2d, is now vaniſhed (Q). With 300, I think there is about ½ dia. of the ſatellite left.

22 30. There now, certainly, is nothing left of the 2d ſat.

Sept. 16. 19 39. The 2d ſat. 1¼ of the projection p. R. (R).

	h.	′	″	°		h.	′	″	°
(F)	11	11	27.	26,8.	(G)	13	34	42.	276,0.
(H)	11	35	5.	36,7.	(I)	9	57	29.	291,0.
(K)	11	7	48.	270,3.	(L)	8	12	24.	157,5.
(M)	14	22	27.	322,9.					

(N) See the obſervation of the 1ſt ſat. Sept. 13.

(O) 10 h. 31′ 13.″ 205°,0. (P) 10 h. 9′ 21″. 334°,6.

(Q) The word vaniſhed is here probably meant to denote its being gone upon the ring, to the projection of which it was approaching 28′ before.

(R) 7 h. 45′ 48″. 224°,9.

22 15. 1 dia. of ♄ from the edge of the R. preceding, and exactly in the line of the R.

23 59. Almoſt 1 dia. of the R. preceding the edge of it.

1 3. About 1¼ dia. of ♄ from the preceding edge.

Sept. 17. 19 48. (S).

Sept. 18. 21 15. The 2d ſat. 1¼ of the projection f. the edge; it is 1 dia. of the 1ſt ſatellite nearer to ♄ than the 1ſt, and a little more ſouth.

22 35. A little leſs than 1 projection f. the edge.

23 14. About ¾ of the projection f. the edge.

0 12. ½ of the projection following, and a little ſouth.

0 27. With 300, the 2d ſat. 1 of its own dia. f. the R.

0 51. With 157, the 2d ſat. cloſe to the R. ſo that no diviſion can be perceived (T).

0 55. With 300, the ſatellite touches the R. and is a little ſouth; its whole dia. is ſtill out.

0 58. With 300, about ¾ of the dia. of the 2d ſat. may yet be ſeen.

Sept. 20. 23 27. The 2d within one of its diameters following the 3d, and a little north (V).

23 51. The 2d is now more ſeparated from the 3d.

0 45. 2 projections f. the edge of the ring.

1 22. 2½ projections f. R.

Sept. 21. 21 20. (W).

Sept. 23. 22 51. Almoſt 1 dia. of the R. f. the edge (X).

(S) 7 h. 50′ 55″. 356,8 invifible.
(T) 12 h. 49′ 10″. 155°,7.
(V) 11 h. 17′ 33″. 50°,6.
(W) 9 h. 6′ 58″. 170°,2 conſequently invifible.
(X) 10 h. 29′ 52″. 81°,0.

Sept.

Sept. 24.. 19 49. The 2d, upon the point of the ring p; but I can ſee no vacancy.

19 56. With 300, the ſame appearance nearly; but the weather is too hazy, and the planet too low to bear it well.

20 45. The 2d ſat. begins now to project a little, and is a little ſouth of the ring (Y).

20 48. I can ſee a diviſion between the 2d ſat. and the R.

Sept. 25. 19 34 to 22 h. 38′ (Z).

Oct. 12. 20 37. The 2d ſat. one full dia. of ♄ following the body (A).

Oct. 15. 20 54. The 2d ſat. 1½ dia. of ♄ f. the body (B).

23 20. About 1½ dia. of ♄ following.

Oct. 16. 20 16. The 2d ſat. ¾ dia. of ♄, or a little leſs p. the body (C).

20 36. ¾ dia. of ♄ p. the body.

22 35. 1¼ dia. of ♄ p. the body.

0 11. 1½ dia. of ♄ p. the body.

Oct. 17. 21 30. The 2d ſat. 1 projection p. the body of ♄; very hazy weather (D).

(Y) 8 h. 20′ 17″. 200°,7.

(Z) From 7 h. 5′ 33″ to 10 h. 9′ 3″, the ſatellite is not mentioned in my obſervations; though by calculation it appears, that its ſituation was from 325°,6 to 342°,3; and that therefore it ought to have been ſeen. I conclude from this that ſome particular cauſe muſt have rendered it inviſible. Moſt probably it ſuffered an occultation from the 1ſt ſatellite, which was ſituated in ſuch a manner as nearly to cover it the whole evening; in this caſe, the obſervation of the 1ſt belongs alſo to the 2d, ſince their diameters would certainly run together ſo as, perhaps, if the occultation was not always central, to form only one ſatellite, of rather a larger diameter than either of them.

	h.	′	″	°
(A)	7	1	39.	42,4.
(B)	7	6	50.	77,7.
(C)	6	25	1.	205,4.
(D)	7	34	53.	343,4.

Oct.

Oct. 18. 20 18. The 2d ſat. near 2 dia. of ♄ f. the body.

21 51. 1¾ dia. of ♄ f. the body.

0 52. It approaches to a conjunction with the 1ſt.

1 25. The 2d ſat. very nearly in conjunction with the 1ſt.

1 38. The conjunction is complete (E). By a figure, the 2d is towards the north of the 1ſt; but they ſeem to be in contact.

Oct. 20. 21 17. The 2d ſat. is emerged ſome time ago, and is now 1¼ of its own diameters from the body of ♄. I perceived the ſatellite as a protuberance before 20 h. 50′ (F).

Oct. 28. 20 58. The preceding arm, on the north ſide, very near to the body, contains a conſiderable ſatellite.

21 5. The 2d ſat. is cloſe to the body, on the p. ſide, to the north of the ring (G).

Oct. 29. 21 49. The 2d about 1⅜ dia. of ♄ f. the body (H).

Oct. 30. 20 53. About 2 dia. of ♄ p. the body (I).

Oct. 31. 21 13. The 2d, ¼ dia. of ♄ following the body.

21 57. There is a complete conjunction between the 2d and 3d. The arm paſſes exactly between them, and points to the 6th. The diſtance between the 2d and 3d is about ½ the dia. of the 3d; the 2d ſatellite being to the north, and the 3d to the ſouth (K).

22 0. I can ſee that the conjunction between the 2d and 3d ſatellites is paſt.

23 13. The 2d ſat. is paſt the conjunction with the 6th.

Nov. 2. 21 6. The 2d ſat. juſt preceding the 3d, but a little more ſouth.

21 44. 2 dia. of ♄ p. the body, and a little more ſouth than the 3d ſatellite (L).

	h.	′	″	°
(E)	11	38	17.	137,2.
(G)	6	26	46.	344,6.
(I)	6	6	57.	245,9.
(L)	6	46	3.	284,1.

	h.	′	″	°
(F)	7	10	8.	15,9.
(H)	7	6	44.	119,8.
(K)	7	6	51.	22,9.

Nov. 3. 22 1. The 2d ſat. 2 dia. of ♄ following the body (M).

Nov. 4. 23 42. The 2d ſat. ¼ dia. of ♄ p. the body (N).

23 57. The dia. of the 2d ſatellite is intirely ſouth of the p. arm.

Nov. 7. 21 28. The 2d and 1ſt ſatellites, about 1 dia. of ♄, or a little farther, p. R.

21 53. The 2d ſat. 1⅛ dia. of ♄ p. the body (O).

Nov. 8. 20 46 to 23 h. 40′ (P).

Nov. 10. 23 30. As the calculation gives it (Q).

Nov. 13. 22 33. On the preceding ſide (R).

Nov. 15. 22 33. The 2d ſat. is upon the preceding arm of the ring about half-way; all its dia. is towards the ſouth (S).

22 49. The 2d is not quite to the end of the R. yet, but keeps advancing.

22 56. ¾ of the projection preceding the body, or ¼ wanting to being at the end of the ring.

Nov. 26. 22 27. The 2d is upon the p. arm; its whole dia. is towards the ſouth (T).

0 28. The 2d is emerged ſome time paſt (V).

Dec. 2. 22 50. The 2d ſat. about 1¾ dia. of ♄ preceding the body (W).

Dec. 5. 0 10. As the calculation gives it (X).

	h.	′	″	°		h.	′	″	°
(M)	6	59	4.	56,9.	(N)	8	35	52.	197,3.
(O)	6	35	24.	220,9.					
(P)	5	24	39.	346,1.					

The firſt part of the evening was not very clear, and afterwards, by the calculation, the ſatellite was inviſible.

	h.	′	″	°		h.	′	″	°
(Q)	8	0	21.	263,4.	(R)	6	51	44.	291,8.
(S)	6	43	53.	194,2.	(T)	5	54	43.	196,5.

(V) To emerge was here probably put for coming off the arm.

(W) 5 h. 54′ 6″. 265°,5. (X) 7 h. 2′ 7″. 306°,3.

Dec.

Dec. 16. 23 59. The 2d, about 1¼ or 1⅓ dia. of ♄ p. the body (Y).

Obſervations on the firſt ſatellite of Saturn.

July 18. 19 50. The following part of the ring of Saturn, which is a very thin lucid line, ends in a bright point like a very faint ſatellite (A).

I ſuppoſe the bright point on the f. part of the ring to be a very ſmall fixed ſtar (B).

20 14. The bright point on the following part of R. ſeems to have its whole dia. towards the north; and in all appearance adheres to the line.

0 48. Poſſibly the bright point on the nf. part of the ring may be one of the ſatellites, and one of the before ſuppoſed ſatellites may be a ſmall fixed ſtar (C).

(Y) 6 h. 7′ 58″. 307°,8.

(A) 11 h 52′ 21″. 146,7. This ſhews, that the bright point was the 1ſt ſatellite.

(B) Being the firſt night of my viewing the ſatellites this year, their places were unknown. The 6th, which was in view, I took for the 1ſt ſatellite; but, the 2d, 3d, and 4th being alſo before me, there remained only the ſuppoſition of ſome ſmall fixed ſtar to account for the bright point.

(C) The motion of the bright point on the ring led me to the ſuppoſition of its being a ſatellite; and, to make room for one, it occurred, that one of the others might be a ſtar: for ſtill the thought of an unknown ſatellite did not happen to ſtrike me. I ſhould have made an attempt to calculate the places of the ſatellites by the manuſcript tables of M. DE LA LANDE, which are now printed in the *Connoiſſance des Temps* for 1791; but as there chanced to be an erratum of one day's motion in the epochæ of all the ſatellites for 1788, of which I was not aware, I had ſo little ſatisfaction from them the year before, that I laid them by as uſeleſs, and reſolved to inveſtigate the epochæ and revolutions of the ſatellites from my own obſervations.

21 15.

21 15. The nf. bright point is advanced towards the body, ſo that it no longer hangs at the far end of the ring (D).

July 23. 19 29. (E).

July 27. 20 27. One dia. of ♄ f. is a ſmall ſatellite (F).

July 28. 19 40. The 1ſt ſat. 1½ dia. of ♄ preceding the body (G).

22 34. The 1ſt ſat. as before.

Aug. 18. 21 11. 1½ dia. of ♄ p. R. (H).

Aug. 28. 0 9. 1¼ or 1⅓ projection f. R. or 1 dia. of ♄ from the body (I).

Aug. 29. 22 18. About 1½ dia. of ♄ p. the body (K).

Aug. 31. 20 54. 1½ dia. of ♄ p. the body (L).

Sept. 8. 22 51. (M).

Sept. 10. 22 49 and 23 h. 4′ (N).

Sept.

(D) 13 h. 17′ 7″. 158°,0. Hence we ſee, that the ſatellite had advanced 11 degrees in its orbit towards ♄, ſince 11 h. 52′ 21″ which agrees with the motion of the bright point.

(E) h. 11 11′ 47″. 15,1. Therefore the 1ſt ſatellite was not viſible.

(F) 11 53 55. 63,5. It was the firſt.

(G) 11 3 7. 247,5. The names of the ſatellites were by this time aſcertained, and I found that the above-mentioned manuſcript tables agreed pretty well with my obſervations this evening.

(H) h. 11 11′ 27″. 294°,3.

(I) h. 13 49′ 42″. 60°,4.

(K) 11 35 5. 236°,0.

(L) 10 3 29. 245°,4.

(M) 11 28 45. 342°,9. Conſequently the ſatellite was inviſible, or at leaſt might eaſily be overlooked, ſo near the body upon the p. arm as it muſt have been ſituated; but there was an obſervation made upon what is called the 1ſt ſatellite, which will be reported hereafter. See obſervations on the 7th ſatellite, Sept. 8.

(N) From 11 h. 18′ 54″ to 11 h. 33′ 51″. 3°,1 to 5°,1. The 1ſt ſatellite was inviſible; but two obſervations were made upon what is called the firſt, which will be ſeen in the obſervations upon the 6th ſatellite. Such miſtakes may eaſily be

Sept. 11. 1 0. The 1ſt ſat. about 2 of its own dia. p. the projection; emerged ſince I looked laſt (O).

1 34. Half a projection p. R.

1 57. $\frac{3}{4}$ of the projection p. R.

Sept. 13. 22 0. There is a ſatellite emerging from the preceding arm; I take it to be the 1ſt (P).

22 6. There are two ſatellites emerging inſtead of one.

22 13. The one that emerged firſt, $\frac{1}{3}$ of the projection p. R.

Sept. 14. 21 55. The 1ſt ſat. $\frac{3}{4}$ of the projection f. R. and a very little ſouth.

23 22. With 300, 1 or $1\frac{1}{2}$ of its own dia. farther from the R. than the 6th, and a little more ſouth (Q).

0 42. $1\frac{1}{2}$ projection f. R.

1 24. $1\frac{1}{2}$ projection f. R.

1 46. Much the ſame as before.

Sept. 16. 19 39. The 1ſt ſat. $\frac{1}{2}$ projection f. R (R).

22 18. $1\frac{1}{2}$ projection f. the edge, and a very little ſouth.

23 59. About 1 dia. of ♄ f. the edge of the R.

1 3. $\frac{3}{4}$ dia. of ♄ f. R.

Sept. 17. 19 48. The 1ſt almoſt 1 projection p. R. (S).

20 38. $1\frac{1}{3}$ projection p. R.

Sept. 18. 21 15. The 1ſt ſat. $1\frac{3}{4}$ projection f. R. or one of its own dia. following the 2d ſatellite (T).

22 35. 2 projections following the R.

made during the time of obſervation, as a few hours will bring one of the inner ſatellites in view; but with ſuch accuracy of calculating the preciſe moment and ſituation of the ſatellites, as has now been uſed, there can be no doubt to which ſatellite an obſervation belongs.

	h.	′	″	°		h.	′	″	°
(O)	13	25	37.	210,7.	(P)	10	18	15.	207,4.
(Q)	11	36	6.	48,5.	(R)	7	45	52.	39,6.
(S)	7	50	55.	231,0.	(T)	9	13	45.	72,8.

0 14. 1¼ projection f. R.

Sept. 20. From 23 h. 24′ to 1 h. 28′ (V). Notwithſtanding my utmoſt endeavour, I could not perceive the 1ſt ſatellite. From the tables I ſurmiſe that it might be under an occultation, or eclipſed by the 3d ſatellite; I looked for it above two hours. It could be neither in the ſhadow of Saturn, nor in that of the ring.

Sept. 21. 21 15. The 1ſt ſat. 2 projections p. R. (W).

22 44. 1½ or 1¾ projection p. R.

Sept. 23. 22 51. ¾ projection p. R. and a very little north. It follows the 6th ſatellite 1 dia. of the 6th (X).

23 55. The 1ſt ſat. almoſt touches the ring; it may want one of its diameters. Clouds interrupted the obſervation.

Sept. 24. 19 49. The firſt ſat. 1¼ projection f. R. or about one of its own dia. f. the 6th.

20 45. The 1ſt ſat. 1 full projection f. R.

22 47. Cloſe to the following projection (Y).

Sept. 25. 19 34. The 1ſt ſat. 1 full projection p. the edge of R. and a little north.

20 41. A little more than ½ projection p. R.

22 38. The 1ſt ſat. has half its dia. projecting towards the north from the ring, on the preceding ſide; its place on the ring is about ⅔ of the projection from the body of ♄. The night is extremely clear (Z).

Oct. 12. 20 37. About ¾ dia. of ♄ p. the body.

21 24. ½ projection of R. preceding the edge, and conſiderably more north.

(V) From 11 h. 14′ 33″ to 13 h. 18′ 13″. 110°,4 to 126°,6.

	h.	′	″	°		h.	′	″	°
(W)	9	1	59.	283,5.	(X)	10	29	52.	316,7.
(Y)	10	21	57.	146,4.	(Z)	10	9	3.	335,5.

22 6. The 1ſt almoſt touches the p. projection.

22 24. It very nearly touches the p. projection.

23 26. ¾ of the projection p. the body of ♄; or, as it were, faſtened upon the projection, ¼ from the end of it (A).

1 8. I ſee nothing of the 1ſt ſat.

Oct. 15. 0 52. The 1ſt ſat. ½ projection from the body (B).

0 59. The diſtance of the 1ſt from the body is almoſt, but not quite, equal to the diſtance of the 1ſt, 3d, and 6th from each other.

1 14. The firſt is nearer to the 3d than the 3d to the 6th.

1 35. The 1ſt and 3d approach to a conjunction.

1 45. The 1ſt and 3d very near their conjunction.

Oct. 16. 0 11. The 1ſt ſatellite ¼ dia. of ♄ f. the body (C).

1 20. ¼ of the projection f. the edge of the R.; the weather remarkably clear. I can ſee the R. very diſtinctly, ſo as to judge with ſafety of the projection.

Oct. 18. 21 7. The 1ſt ſat. is lately emerged from the body of ♄ on the f. ſide.

21 12. The emerged ſat. 1 of its own dia. f. the body of ♄.

21 32. Above 2 of its own dia. following ♄.

21 51. The 1ſt ſat. ⅜ dia. of ♄ f. the body.

22 36. Very nearly clear of the f. projection (D). By hiding the planet behind the field-bar very carefully, I can ſee the projection of the R. very well on the f. ſide. The preceding projection cannot be diſtinguiſhed ſo well on account of the ſatellites (E) that are upon it.

0 52. The 1ſt ſat. approaches to a conjunction with the 2d.

1 25. The 1ſt and 2d ſat. very nearly in conjunction.

	h.	′	″	°	
(A)	9	50	11.	336	,0.
(B)	11	4	11.	198	,0.
(C)	10	19	22.	22	,8.
(D)	8	36	47.	30	,8.

(E) The 6th and 7th.

1 38.

1 38. The conjunction is complete.

Oct. 20. 20 5. The 1ſt ſat. $\frac{1}{8}$ dia. of ♄ f. the body (F).

20 50. It draws towards a conjunction with the 6th ſat. diſtance 1 dia. of the 1ſt. The 1ſt is a little towards the north.

21 26. I can juſt ſee a very ſmall diviſion between the 1ſt and the 6th.

21 51. There is a perfect conjunction between the 1ſt and 6th.

Oct. 28. 21 1. The 1ſt ſat. 1 dia. of ♄ f. the body.

21 50. It draws towards a conjunction with the 6th, diſtance 1 full dia. of the 1ſt (G).

Oct. 29. 21 49. The 1ſt about $\frac{7}{8}$ dia. of ♄ p. the body (H).

Oct. 30. 20 53. The 1ſt ſat. $\frac{3}{4}$ dia. of ♄ f. the body (I).

Oct. 31. 21 13. $\frac{3}{4}$ dia. of ♄ p. the body (K).

23 13. $\frac{3}{8}$ dia. of ♄ p. the body; its whole dia. ſeems to be north of the arm.

Nov. 2. 23 26. I ſuppoſe the 1ſt ſat. to be upon the f. arm.

0 8. The f. arm contains a lucid point at the diſtance of $\frac{1}{8}$ dia. of ♄ f. the body.

0 34. The 1ſt ſat. almoſt $\frac{1}{4}$ dia. of ♄ f. the body (L).

Nov. 3. 22 3. The preceding arm is loaded in two places; at the far end, and about the middle.

23 48. The 1ſt ſat. $\frac{1}{2}$ dia. of ♄ p. the body (M); there ſeems to be another cloſely following it (N).

	h.	′	″	°
(F)	5	58	20.	31,3.
(G)	7	11	39.	127,0.
(H)	7	6	44.	317,1.
(I)	6	6	57.	139,8.
(K)	6	22	59.	332,7.
(L)	9	35	35.	19,7.
(M)	8	45	47.	203,8.

(N) It was the 6th.

0 10. I can diſtinguiſh the two ſatellites that follow one another upon the arm; the diſtance between them is $\frac{1}{2}$ dia. of the ſmalleſt of them.

Nov. 4. 22 14. The 1ſt ſat. $\frac{1}{4}$ dia. of ♄ f. the body (O).

22 57. The dia. of the 1ſt ſat. is north of the arm.

Nov. 7. 21 28. The 1ſt and 2d about 1 dia. of ♄, or a little more, p. the body (P).

21 53. The 1ſt ſatellite $1\frac{1}{4}$ dia. of ♄ p. the body.

Nov. 8. 20 46. $\frac{7}{8}$ dia. of ♄ f. the body (Q).

Nov. 10. 23 30. The 1ſt as the calculation gives it (R).

Nov. 13. 22 33. On the preceding ſide (S).

Nov. 15. 22 33. About $\frac{3}{4}$ dia. of ♄ p. the body (T).

Nov. 16. 22 50. Upon the end of the f. arm (V).

Nov. 21. 0 54. $\frac{3}{4}$ dia. of ♄ f. the body (W).

Dec. 2. 22 49. The 1ſt ſat. about 1 of its own dia. p. the 6th.

23 38. It is paſt the conjunction with the 6th, which it now follows, and it is a little more north than the 6th.

0 22. The 1ſt is equally diſtant from the following 6th, and the preceding 3d (X).

0 50. The 1ſt and 3d are in conjunction, with a little ſpace between them, the 1ſt being to the north.

Dec. 5. 0 8. (Y).

(O) 7 h. 8′ 6″. 21°,5.

(P) 6 h. 10′ 28″. 226°,0. By the equal diſtance which is mentioned, it appears, that the 1ſt and 2d ſatellites were in conjunction; and this agrees alſo with the next obſervation, compared with that of the 2d ſat. Nov. 7th.

	h.	′	″	°		h.	′	″	°	
(Q)	5	24	39.	50,8.	(R)	8	0	21.	92,7.	
(S)	6	51	44.	295,9.	(T)	6	43	53.	316,2.	
(V)	6	56	55.	148,6.	(W)	8	40	57.	35,9.	
(X)	7	25	51.	323,5.	(Y)	7	0	7.	172,1	inviſible.

Dec.

Dec. 16. 23 59. The 1ſt ſat. a little more than 1 dia. of ♄ f. the body (Z).

Dec. 24. 0 5. (A).

Dec. 25. 1 36. The 1ſt ſat. is upon the end of the f. arm (B).

Obſervations on the ſixth ſatellite of Saturn.

July 18. 19 50. The *firſt ſatellite* of ♄ exactly in a line of the R. preceding (A).

July 27. 20 24. Upon the ff. part of the ring are two ſmall bright points; the largeſt, to the ſouth, is neareſt to the body (B), and the ſmalleſt, to the north, is at the farther end (C).

Aug. 28. 23 26. With the 40-feet reflector, I ſee the five known ſatellites of Saturn, and alſo another exactly in a line with the ring, interpoſed between the 2d ſatellite and the ring on the preceding ſide, while the 1ſt, 3d, 4th, and 5th are on the following one. It has ſo much the appearance of the other ſatellites, and ranges ſo well with them, that I have not a moment's doubt but that it is a ſixth ſatellite. It is leſs bright

	h.	′	″	°		h.	′	″	°	
(Z)	6	7	58.	102,4.	(A)	5	42	33.	184,3	inviſible.
(B)	7	9	23.	26,3.						

(A) By computation for 11 h. 52′ 21″ we find, that the 6th ſat. was 302,4; which is exactly in the place where a ſatellite called the firſt was obſerved; but it appears alſo from the calculation which has been given in the note A of the 1ſt ſatellite, that this obſervation cannot belong to the real 1ſt; the 6th ſatellite therefore was ſeen this evening without being known; and this explains all the difficulties which occurred with regard to the real 1ſt ſatellite. See obſervations on the 1ſt ſatellite, July 18.

(B) It was the 2d ſatellite. See obſervation on the 2d ſatellite, July 27.

(C) 11 h. 50′ 56″. 147°,1, which agrees exactly with the place of the 6th ſatellite.

than the reſt, but ſeems to have light enough to be ſeen by my 20-feet teleſcope.

0 9. 20-feet reflector. The new ſatellite $\frac{4}{5}$ of the projection of the ring preceding the edge of the R. (D).

0 20. A very ſmall ſtar about 60° ff. ♄, and $1\frac{1}{4}$ dia. of ♄ diſtant from the body (E).

1 16. The ſmall ſtar is gradually left behind ſo as now to make an angle of about 35° ff. ♄; while at the ſame time the planet has carried along with him the new ſatellite.

1 24. The 6th ſatellite $\frac{2}{3}$ of the projection of the R. p. the ring.

1 46. The ſame ſmall ſtar is now only about 25° ff. ♄.

1 49. The new ſatellite is now not much more than $\frac{1}{2}$ the projection from the ring.

2 2. Saturn is gone on, in a retrograde order with reſpect to the ſmall ſtar, and has carried along with him the new diſcovered ſatellite.

Sept. 8. 22 30. The new or 6th ſat. $\frac{4}{5}$ of the projection of the R. directly preceding (F).

Sept. 10. 22 49. The *firſt ſatellite* leſs than the projection from the following arm; extremely faint (G).

23 4. The *firſt ſatellite* the length of the projection following the arm; it is ſo faint that I cannot expect to ſee the new ſatellite (H).

Sept.

(D) 13 h. 29′ 42″. 294°,2.

(E) This ſtar was immediately taken notice of, to verify the diſcovery of the 6th ſatellite.

(F) 11 h. 7′ 48″. 279°,2.

(G) 11 h. 18′ 54″. 86°,8. Which agrees perfectly with the 6th ſatellite, though it is here by miſtake called the 1ſt.

(H) As I miſtook the 6th ſatellite for the 1ſt, it was natural enough to find it very

Sept. 14. 21 59. I think I perceive a ſatellite between the 1ſt and the following projection cloſe to the ring. 300 leaves it doubtful (I).

22 23. The 6th ſat. ¾ of the projection f. R.; ſo cloſe to the 1ſt that it requires great attention to be diſtinguiſhed. With 300, the 6th is 1 or 1½ dia. of the 1ſt ſat. nearer the R. than the 1ſt, and a little more north than the 1ſt, that is to ſay, very exactly in the line of the ring.

23 45. With 460, the 6th ſat. is very near one whole projection f. R.

0 42. The 6th ſat. 1 full projection f. R. (K).

1 24. The new ſat. 1 projection f. R.

1 46. Very nearly, but not quite, 1 projection f. R.

Sept. 16. 22 18. The *third ſatellite* ½ the projection from the preceding edge of R.

22 25. The *third ſatellite* is extremely ſmall, and hardly to be ſeen; but I have no doubt.

23 59. *The third ſatellite* a little more than ½ the projection preceding the edge.

0 16. *The third* much leſs than the 1ſt and 2d, partly owing to its proximity to the planet; but probably there may be an apparent change of magnitude from a revolution upon its axis.

1 3. *The third* about ¾ of the projection p. R. (L).

Sept.

very faint, and of courſe to ſuppoſe that the night was not clear enough to ſee the 6th, while at the ſame time I was making an obſervation on that very ſatellite. But it muſt here be remembered, that the time of its revolution was not yet well aſcertained.

(I) The calculation for 10 h. 13′ 20″ gives 46°.0, which ſhews that the ſatellite was there.

(K) 12 h. 55′ 53″. 75°,7.

(L) The 6th ſat. was this evening miſtaken for the 3d; but the calculation for

Sept. 17. 19 52. The 6th ſat. 1 projection, or rather more f. the projection; extremely faint.

20 38. 1 full projection following (M).

22 55. ½ projection f. and a little ſouth; extremely ſmall.

23 49. ⅓ projection f. and a little ſouth.

0 58. ⅓ projection following.

1 46. Near ¾ of the projection following (N).

Sept. 21. 21 10. The 6th ſat. 1 full projection f. R.; much fainter than the 1ſt; hazy weather.

21 20. 1 full projection f. the edge, and exactly in the line of the ring. I ſee it very well; it is leſs than the 1ſt.

22 9. 1¼ of the projection f. the edge (O).

22 39. Nearly 1¼ projection f. the edge of the R. exactly in the line of the R.

Sept. 23. 22 51. The 6th ſat. 1 projection p. the edge; or 1 of its own diameters p. the 1ſt (P).

Sept. 24. 19 46. I ſuſpect the new or 6th ſatellite to be 1 projection f. the edge (Q).

19 49. The 6th is very near 1 projection f. the edge; it precedes the 1ſt ſat. about 1 dia. of the 1ſt.

20 45. The 6th a little more than ½ projection f. the edge.

13 h. 8′ 59″, which gives 243°,7, ſhews that theſe obſervations belong to the 6th; and therefore explains all the difficulty about the ſuppoſed change of magnitude of the 3d.

(M) 8 h. 40′ 47″. 97°,6.

(N) The eſtimation ¾ is probably a miſtake in writing down, and ſhould have been ¼; perhaps alſo ſome change in the atmoſphere, or other circumſtance, may have induced an error of eſtimation, which, in ſuch minute objects, will now and then take place.

(O) 9 h. 55′ 50″. 82°,5.

(P) 10 h. 29′ 52″. 254°,3.

(Q) 7 h. 21′ 28″. 122°,6.

Sept.

Sept. 25. 22 36. The 6th almoſt ¾ projection f. the edge of the R.

23 42. The 6th ſat. very nearly 1 projection f. R. (R).

23 52. The 6th ſat. is much larger than the 7th.

Oct. 12. From 22 h. 6′ to 1 h. 8′ (S).

0 58. *The ſeventh ſat.* extremely ſmall upon the point of the preceding projection, and a little towards the ſouth.

1 20. The diſtance of *the ſeventh* increaſes. The ſatellite ſeems to be clear of the projection; but I can ſee no diviſion yet; its whole dia. ſeems to be to the ſouth; I ſee it full as well as I ſaw the 6th, or rather better (T).

1 35. *The ſeventh* is clear of the projection.

Oct. 15. 20 47. The 6th ſat. 1 dia. of ½ p. the body.

21 34. The 6th about ¾ of the projection p. the edge of R. or very near 1 dia. of ½ p. the body; juſt ff. the 3d; it is in the line of the R. (V).

22 25. The 6th ſat. will be in conjunction with the 3d in a very ſhort time, the 3d being ſtill a little p.

22 39. The conjunction is ſo complete now, that I have loſt the 6th. The 3d, however, appears to be a little lengthened out towards the ſouth. Diſtance of the conjoined ſatellites

(R) 11 h. 12′ 52″. 67°,7.

(S) There were five obſervations made upon the 6th ſatellite, but they belong to the 7th. There were alſo three obſervations made upon the 7th which belong to the 6th, and are here given; we are to obſerve, that the revolution of the 7th was not yet aſcertained, and that, conſequently, a miſtake of one new ſatellite for another could eaſily be made.

(T) 11 h. 43′ 53″. 220°,9. The calculation of its place ſhews plainly that it was the 6th; and the remark in this obſervation of its being brighter than the other ſatellite perfectly agrees with the calculation.

(V) 7 h. 46′ 43″. 246°,0.

barely 1 dia. of ½ from the body; or juſt 1 dia. of ½, including the dia. of the 3d ſat.

23 59. The 6th near 2 diameters of the 3d ſatellite paſt the conjunction.

0 59. The 6th, the 3d, and the firſt ſatellites are at equal diſtances from each other.

1 3. The 6th is one dia. of ½ from the body.

1 39. The 6th nearly 1 dia. of ½ p. the body.

Oct. 16. 20 16. The 6th ſat. ⅔ dia. of ½ f. the body; too low to be very accurate.

20 36. The 6th one full projection f. the body; extremely faint.

20 50. The 6th one projection f. the body.

21 11. The 6th ¾ of a projection f. the body.

21 55. The 6th ½ projection f. the body, or a little leſs (W).

22 5. The 6th advances towards a contact with the f. part of the body; I can, however, ſtill look between them.

22 18. I can ſtill ſee between the planet and the 6th ſat.

22 22. The 6th leſs than its own dia. from the planet.

22 25. It is in contact with the body of ½.

22 41. I can ſtill perceive the 6th ſat.

22 44. The 6th is not quite vaniſhed.

22 47. The ſatellite is no longer viſible.

Oct. 17. 21 30. The 6th ſat. ½ projection f. the body, very hazy weather (X).

Oct. 18. 20 40. The 6th ſat. is emerging from behind the 3d.

20 46. The 6th ſat. which emerged from behind the 3d is a little north of the line of the ring, and of the 3d.

(W) 8 h. 3′ 44″. 151°,9. (X) 7 h. 34′ 53″. 49°,4.

5

21 36. The 6th is going towards ♄, and is about ¾ dia. of ♄ preceding the body.

22 51. ¾ dia. of ♄ p. the body.

22 26. The 6th about ⅝ dia. of ♄ p. the body.

22 40. The 6th ½ dia. of ♄ p. the body.

23 17. The 6th approaches to a conjunction with the 7th.

23 37. The conjunction of the 6th and 7th ſatellites is paſt. The ſatellites are, however, too near the planet to ſee exactly how they are placed.

0 12. The 6th a little more than 1 of its own dia. p. the body of ♄ (Y).

Oct. 20. 20 5. The 6th ſat. 1¼ dia. of ♄ f. the body (Z).

20 50. The 6th and 1ſt ſatellites are drawing towards a conjunction; diſtance between them 1 dia. of the 1ſt ſat.

21 26. I can juſt ſee a very ſmall diviſion between the 6th and the 1ſt.

21 51. There is a perfect conjunction between the 6th and the 1ſt.

22 22. The 6th ſat. appears again.

22 43. The 6th is in the middle, between the 1ſt and 2d ſatellites.

23 50. I perceive the 6th ſat. near the 3d towards the 2d; and on the ſouth of the line that joins the 3d and 2d; but nearer the 3d than the 2d.

Oct. 28. 20 58. The 6th ſat. about ½ projection f. the edge of the R.

21 5. The 6th about ¾ dia. of ♄ f. the body (A).

Oct. 29. 21 49. The 6th juſt f. the 1ſt (B).

	h.	′	″	°		h.	′	″	°
(Y)	10	12	31.	341,0.	(Z)	5	58	20.	100,0.
(A)	6	26	46.	47,5.					
(B)	7	6	44.	317,5.	For the place of the 1ſt, ſee the 1ſt ſat. Oct. 29.				

Oct.

Oct. 30. 20 55. I ſuſpect the 6th on the edge of the p. arm; but moon-light is too ſtrong.

23 44. The 6th ſat. $\frac{3}{4}$ projection p. the edge of the R.

23 55. The 6th ſat. $\frac{7}{8}$ dia. of ♄ p. the body.

0 42. The 6th ſat. $1\frac{1}{8}$ dia. of ♄ p. the body (C).

Oct. 31. 21 13. The 6th ſat. $\frac{7}{8}$ dia. of ♄ p. the body.

21 57. The f. arm of the R. paſſes between the 3d and 2d ſatellites, and points to the ſixth.

23 57. The 6th full $\frac{1}{2}$ dia. of ♄ following the body (D).

Nov. 2. 21 44. The 6th ſat. $1\frac{1}{8}$ dia. of ♄ p. the body, and a little north; extremely faint (E).

22 17. The 6th ſat. 1 full dia. or $1\frac{1}{16}$ dia. of ♄ p. and a very little north.

22 53. The 6th ſeems to be ſtill $1\frac{1}{16}$ dia. of ♄, or rather more, p. the body, but the weather is hazy and foggy.

23 15. The 6th ſat. 1 dia. of ♄ p. the body.

23 27. The 6th ſat. $\frac{4}{5}$ of the projection p. the edge or very near 1 dia. of ♄ p. the body.

0 15. The 6th ſat. $\frac{7}{8}$ dia. of ♄ p. the body.

0 58. The 6th ſat. is ſtill clear of the p. arm.

1 16. The 6th ſat. $\frac{5}{8}$ dia. of ♄ p. the body; a very little p. the edge of the ring, and a little north.

Nov. 3. 22 3. The p. arm is loaded in two places, at the far end and about the middle (F.)

23 54. There ſeems to be a ſatellite cloſely following the 1ſt (G).

(C) 9 h. 55′ 20″. 250°,9.
(D) 9 h. 6′ 32″. 144°,9.
(E) 6 46 3. 284°,7.
(F) They were the 1ſt and 6th ſatellites. See 1ſt ſat. Nov. 3.
(G) 8 h. 51′ 46″. 210°,5.

0 10.

0 10. The diſtance between the two ſatellites upon the arm is half the dia. of the ſmalleſt.

Nov. 4. 22 17. The 6th ſatellite $\frac{7}{8}$ dia. of ♄ f. the body (H).

23 48. The 6th about $\frac{3}{4}$ or $\frac{7}{8}$ dia. of ♄ following the body.

Nov. 7. 21 28. The 6th towards the end upon the f. arm (I).

22 39. The 6th is drawn a little nearer towards ♄.

Nov. 8. 20 46. The 6th about $\frac{1}{2}$ dia. of ♄ f. hazy weather, I do not ſee it well enough to eſtimate its diſtance very exactly.

21 16. The 6th ſat. $\frac{5}{8}$ dia. of ♄ f. the body.

22 2. The 6th ſat. $\frac{7}{8}$ dia. of ♄ f. the body.

23 40. The 6th ſat. $1\frac{1}{8}$ dia. of ♄ f. the body (K).

Nov. 9. 21 42. The 6th ſat. $\frac{5}{8}$ dia. of ♄ p. the body, and a little north (L).

Nov. 10. 21 33. The p. arm, near the end, ſeems to contain a ſat. probably the 6th.

21 39. The 6th full $\frac{3}{8}$ dia. of ♄ p. the body; almoſt intirely to the ſouth of the arm (M).

22 28. The 6th is clear of the p. arm, and is about $\frac{5}{8}$ dia. of ♄ p. the body.

23 27. The 6th almoſt $\frac{7}{8}$ dia. of ♄ p. the body.

0 10. The 6th ſat. 1 full dia. of ♄ p. the body.

Nov. 13. 22 33. The 6th ſat. on the p. ſide (N).

Nov. 15. 22 33. The 6th ſat. about 1 dia. of ♄ f. the body (O).

Nov. 19. 21 55. The 6th ſat. $\frac{7}{8}$ dia. of ♄ f. the body (P).

	h.	′	″	°
(H)	7	11	6.	94,8.
(K)	8	18	11.	78,0.
(M)	6	9	39.	220,1.
(O)	6	43	53.	100,0.

	h.	′	″	°
(I)	6	10	28.	151,9.
(L)	6	16	34.	318,7.
(N)	6	51	44.	296,0.
(P)	5	50	17.	61,1.

Nov.

Nov. 21. 0 54. The 6th at a little diſt. p. the edge of the ring (Q); cloudy weather.

Nov. 25. 1 21. The 6th about ¾ dia. of ½ following the body (R).

1 27. The 6th about ½ the projection preceding the edge of the projection.

Nov. 26. 22 22. The 6th near 1 dia. of ½ f. the body (S).

0 30. Very nearly in conjunction with the 4th.

Nov. 30. 23 47. The 6th full ⅞ dia. of ½ f. the body; about 2½ dia. of the 3d ſat. p. the 3d (T).

Dec. 2. 22 49. The 6th about ¾ dia. of ½ p. the body; about 1 dia. of the 1ſt f. the 1ſt.

23 38. The 6th ſat. is paſt its conjunction with the 1ſt, which it now precedes.

0 22. The 6th, the 1ſt, and 3d ſatellites, are nearly at equal diſtances from each other.

0 52. The 6th ſat. nearly 1 dia. of ½ p. the body (V).

Dec. 5. 0 8. The 6th ſat. ½ projection p. the arm (W).

Dec. 15. 0 35. The 6th about ¾ dia. of ½ f. the body, and a little north (X).

Dec. 16. 23 59. The 6th full ⅝ dia. of ½ p. the body (Y).

Dec. 24. 0 5. The 6th ſat. 1⅛ dia. of ½ p. the body; or 1¼ projection p. the edge of R. (Z).

	h.	′	″	°	
(Q)	8	40	57.	257,8.	
(S)	5	49	44.	100,1.	
(V)	7	55	46.	259,4.	
(X)	6	47	48.	62,0.	
(Z)	5	42	33.	254,2	

	h.	′	″	°
(R)	8	52	10.	230,7.
(T)	6	58	48.	83,6.
(W)	7	0	7.	317,3.
(Y)	6	7	58.	317,3.

Obſervations

Observations on the ſeventh ſatellite of Saturn.

Sept. 8. 22 51. The *firſt ſatellite* ½ the dia. or a little leſs of the projection ſf. R. (A).

Sept. 14. 1 29. A ſuppoſed 7th ſat. exceſſively faint, ½ projection p. R. exactly in the line of the R. fainter than the laſt new one.

1 46. The ſuppoſed 7th half a projection p. the R. (B).

Sept. 17. 21 0. A ſecond new ſatellite exceſſively faint, ½ projection p. the edge of the R. (C).

22 55. The new, or 7th ſat. ¼ projection p. R. ſo exceſſively ſmall that, if I had not ſeen it before, it would have been impoſſible to perceive it now.

(A) Not being acquainted with more than ſix ſatellites, and having the 2d, 3d, 4th, 5th, and 6th in view, it was natural enough to call the remaining one, on which this obſervation is made, the 1ſt; but from the note M of the 1ſt ſat. it appears, that it could not be in the place where this was ſeen; and by calculating from the tables of the 7th ſat. we have its place for 11 h. 28′ 45″. 106°,0, which agrees exactly with the ſituation pointed out. From a figure, it appears, that the ſat. was extremely ſmall, and leſs than half its diameter ſouth of the line of the R.

(B) I was now on the look-out for very ſmall ſtars that were in any ſituation likely to be ſatellites of ♄, and always noticed them: for inſtance, "Sept. 11. "20 h. 42′. A ſuppoſed 7th ſat. exactly in the line of the R. or a very little "ſouth, exceſſively faint, only to be ſeen when I hide ♄ by the field bar. Sept. "14. 20 h. 40′. The 7th of Sept. 11. is left in its place." So here this ſuppoſed 7th is marked down, and Sept. 16. 20 h. 13′ I find it is ſaid, that "the "ſuppoſed 7th of the 14th is a ſmall fixed ſtar, left in the place where it was that "evening; but as the configuration of ſtars which pointed out this ſuppoſed 7th was very coarſe, and hardly ſufficient to determine the place, and as by calculation it appears, that the 7th ſatellite was in the ſituation where this obſervation places it, at 13 h. 59′ 43″, *viz.* 278°,2, it is probable enough, that I ſaw the real ſatellite this evening. (C) 9 h. 2′ 43″. 265°,6.

23 1. After a more attentive obſervation and hiding the planet, I ſee the 7th ſat. is not leſs than ½ projection p. R.

23 31. Forty-feet reflector. I ſee ſix ſatellites at once, and being perfectly aſſured that the 2d is inviſible, it becomes evident that Saturn has ſeven ſatellites. This new ſat. is exceſſively ſmall.

Sept. 18. 22 4. The new ſat. near ½ projection p. R. and a little ſouth, but ſo faint that I hardly perceive it (D).

22 36. I cannot perceive the new ſatellite with the utmoſt attention (E). Indeed it was ſo faint before, that I almoſt entertained a doubt of its reality.

Sept. 25. 23 48. The 7th ſat. I believe is between the 6th and the R. or ⅓ projection f. the edge (F).

23 52. I ſee it very plainly; it is much ſmaller than the 6th; I have many times this evening before ſuſpected it, but the weather has been too hazy.

Oct. 12. 22 6. *The ſixth ſat.* (G) cloſe to the f. projection, and a little north.

22 13. I ſee *the ſixth ſat.* very well; but the projection is too faint to eſtimate the diſtance by it with any accuracy.

22 24. *The ſixth* being nearer to Saturn on the f. ſide than the 1ſt on the preceding, muſt be quite cloſe to the f. projection, or touching it.

(D) 10 h. 2′ 37″. 303°5.

(E) From the calculated place 10 h. 34′ 32″. 312°,0, we ſee, that the ſatellite was drawn upon the arm, and therefore might eaſily be overlooked, eſpecially as its revolution was unknown.

(F) 11 h. 18′ 48″. 117°,9.

(G) The ſatellite is here called the 6th, and we have ſeen before, in the note (S) of the 6th ſat. that the 6th was called the 7th; but the tables of theſe ſatellites leave no doubt to which of them the obſervations belong.

Oct. 12. 23 35. *The ſixth ſat.* 1 projection, or perhaps a little leſs, f. the body; I ſee it with great difficulty, but have no doubt (H).

1 8. I ſee nothing of *the ſixth* (I).

Oct. 16. 20 23. The 7th ſat. ½ projection f. the body; that is, the ſat. is upon the middle of the arm (K).

20 36. The 7th a little more than ¼ projection f. the body; extremely faint.

20 50. The 7th ſat. 1 of its own dia. f. the body of ♄.

21 11. The 7th is very nearly in contact with the body.

21 15. I can ſtill perceive the 7th ſat. by means of the field bar hiding the planet.

21 55. The 7th is gone.

1 29. I have a ſtrong ſuſpicion of a ſat. upon the p. projection not far from the end of it (L).

Oct. 18. 21 25. I am pretty ſure the ſat. is about 1 of its own dia. p. ♄ (M).

21 26. Very clear. I ſee the 7th ſat. very plainly.

21 35. About 2 of its own dia. from the body of ♄, and a little north of the R.

21 43. The 7th ſat. ¾ projection p. the body.

21 51. The 7th ſat. ¼ dia. of ♄ p. the body.

22 26. The 7th ſat. ¼ dia. of ♄ p. the body.

(H) The calculated place 9h. 59′ 10″. 111°,2, gives the ſatellite farther from the arm than the obſervation; but as this alſo mentions the ſat. was ſeen with difficulty, the interval might appear leſs than it would have done in a very clear view of the ſat.

(I) The leaſt change in the atmoſphere would make the ſat. inviſible; and by the tables it alſo was now very nearly going upon the ring.

	h.	′	″	°
(K)	6	31′	59″.	144°,4.
(L)	11	37′	9″.	225°,3.
(M)	7	25	58.	202°,7.

22 40. The 7th ſeems ſtill to be where it was.

23 17. The 7th approaches to a conjunction with the 6th.

23 37. The conjunction of the 7th and 6th ſatellites is paſt. They are too near the planet to ſee exactly how they are placed.

0 20. The 7th ſat. $\frac{5}{8}$, or near $\frac{3}{4}$, dia. of ½ p. the body.

0 24. The 7th is clear of the projection.

0 36. The 7th near $\frac{1}{2}$ projection p. the edge of the ring. I ſee the R. well enough to eſtimate by it.

0 59. The 7th ſat. $\frac{3}{4}$, or nearly $\frac{7}{8}$, dia. of ½ p. the body.

1 21. The 7th ſat. $\frac{7}{8}$ dia. of ½ p. the body (N).

Oct. 20. 21 26. I have a glimmering ſight of the 7th ſat.

21 56. The 7th is perfectly detached from the p. arm.

23 5. The ſat. $\frac{3}{4}$ of the dia. of ½ p. the body, or thereabout.

23 37. The 7th extremely faint, near 1 dia. of ½ p. the body; but the eſtimation of the diſtance is not very exactly to be had, as I am obliged to hide the planet when I ſee the ſat. There is a high wind, and the air being dry, the teleſcope does not act ſo well as it did 1½ hour ago.

0 8. The 7th ſat. $\frac{3}{4}$ dia. of ½ p. the body; or $\frac{1}{3}$ (or nearly $\frac{1}{2}$) projection p. R. I ſee the ring very plainly (O).

0 20. The 7th ſat. about $\frac{3}{4}$ dia. of ½ p. the body.

1 20. The 7th ſat. about $\frac{5}{8}$ dia. of ½ p. the body.

Nov. 4. 22 23. The p. arm ſeems to be loaded about $\frac{3}{8}$ dia. of ½ from the body (P).

Nov. 7. 22 0. The 7th ſat. $\frac{7}{8}$ dia. of ½ p. the body, exceſſively ſmall; but I ſee it extremely well, and can keep it in view; it is juſt following the 1ſt and 2d ſat.

	h.	′	″	°		h.	′	″	°
(N)	11	21	20.	265,1.	(O)	10	0	40.	287,9.
(P)	7	17	5.	214,3.					

22 9. I ſee the 7th extremely well, notwithſtanding its ſmallneſs.

22 39. The 7th about $\frac{7}{8}$ dia. of ♄ p. the body (Q).

23 12. The 7th nearly the ſame as at 22 h. 39′, or perhaps a little nearer to ♄.

Nov. 8. 21 17. The 7th is clear of the p. arm; but I do not ſee it well enough yet to eſtimate how much.

22 0. The 7th ſat. $\frac{3}{4}$ dia. of ♄ p. the body (R).

23 31. The 7th is upon the p. arm I ſuppoſe, for the weather is now very clear, and I ſee it no longer. There is a ſmall protuberant point on the arm, which I take to be the ſatellite; but, as it has been cloudy, I have not been able to follow it ſo as to ſee it go on ſince 22 h. 2′.

23 40. I ſee the 7th upon the arm.

Nov. 10. 21 39. The p. arm is a little gouty, not quite $\frac{3}{8}$ dia. of ♄ preceding the body (S).

Nov. 15. 22 27. The 7th ſat. is clear of the f. projection (T).

22 39. The 7th ſat. between the 6th and the projection of the R. and a very little to the north.

22 44. The 7th conſiderably leſs than the 6th; I ſee it however very well, notwithſtanding the difficulty of its ſituation.

22 56. By a figure, at a conſiderable diſtance following.

Nov. 16. 22 50. The 7th leſs than 1 dia. of the 1ſt following the firſt, and a little north (V).

Nov. 29. 0 38. A ſmall luminous point on the f. arm (W).

	h.	′	″	°	
(Q)	7	21	16.	281,4.	
(S)	6	9	39.	328,5.	
(V)	6	56	55.	112,8.	
(R)	6	38	27.	291,9.	
(T)	6	37	54.	85,7.	
(W)	7	53	35.	53,3.	

Dec.

Dec. 2. 23 38. I ſuſpect the 7th to be juſt detached from the f. arm (X).

o 24. I cannot ſee the 7th, though I tried often for it.

o 56. The 7th ſat. is not viſible.

From the obſervations on the ſeven ſatellites of Saturn that have been here delivered, and cloſely compared with their calculated places, it appears evidently that the revolutions of theſe ſatellites are ſo well aſcertained, that we may, without heſitation, determine that no phænomenon on the ring of Saturn, in the ſhape of lucid ſpot, protuberant point, or latent ſatellite, can be occaſioned by any of them, when, upon computation, we find that the place of the ſatellite differs from that where ſuch appearances were obſerved. In conſequence of this deduction, I found, that the obſervations, which will be given preſently, could not be explained by any of the known ſatellites; it remained, therefore, to be examined to what cauſe to aſcribe the appearance of ſuch lucid ſpots.

The firſt idea that occurred was that of another ſatellite, ſtill cloſer to the ring than the ſeventh; and if a revolution, ſlower than about 15 hours and a quarter, could have been found, which would have taken in the moſt material places in which bright ſpots were ſeen, I ſhould have continued of opinion that an eighth ſatellite, exterior to the ring, did exiſt, notwithſtanding more obſervations had been wanting to put the matter out of all doubt. But this being impracticable, I examined, in the next place, what would be the reſult if theſe ſuppoſed ſatellites, or protuberant points, were attached to the plane or edge of the ring.

(X) 6h. 41′ 59″. 100°,0.

As

As obſervations, carefully made, ſhould always take the lead of theories, I ſhall not be concerned if ſuch lucid ſpots as I am now going to admit, ſhould ſeem to contradict what has been ſaid in my laſt Paper, concerning the idea of inequalities, or protuberant points. We may however remark, that a lucid, and apparently protuberant point, may exiſt without any great inequality in the ring. A vivid light, for inſtance, will ſeem to project greatly beyond the limits of the body upon which it is placed. If therefore the luminous places on the ring ſhould be ſuch as proceed from very bright reflecting regions, or, which is more probable, owe their exiſtence to the more fluctuating cauſes, of inherent fires acting with great violence, we need not imagine the ring of Saturn to be very uneven or diſtorted, in order to preſent us with ſuch appearances as will be related. In this ſenſe of the word, then, we may ſtill oppoſe the idea of protuberant points, ſuch as would denote immenſe mountains of elevated ſurface.

On comparing together ſeveral obſervations, a few trials ſhew that the brighteſt and beſt obſerved ſpot agrees to a revolution of 10 h. 32′ 15″,4; and, calculating its diſtance from the center of Saturn on a ſuppoſition of its being a ſatellite, we find it 17″,227, which brings it upon the ring. It is therefore certain, that unleſs we ſhould imagine the ring to be ſufficiently fluid to permit a ſatellite to revolve in it, or ſuppoſe a notch, groove, or diviſion in the ring, to ſuffer the ſatellite to paſs along, we ought to admit a revolution of the ring itſelf.

The denſity of the ring indeed may be ſuppoſed to be very inconſiderable by thoſe who imagine its light to be rather the effect of ſome ſhining fluid, like an aurora borealis, than a reflection from ſome permanent ſubſtance; but its diſapparition in general, and in my teleſcopes its faintneſs when turned edge-

ways, are in no manner favourable to this idea. When we add alſo, that this ring caſts a deep ſhadow upon the planet, is very ſharply defined both in its outer and inner edge, and in brightneſs exceeds the planet itſelf, it ſeems to be almoſt proved, that its conſiſtence cannot be leſs than that of the body of Saturn; and that conſequently, no degree of fluidity can be admitted ſufficient to permit a revolving body to keep in motion for any conſiderable time.

A groove might afford a paſſage, eſpecially as on a former occaſion we have already conſidered the idea of a divided ring. A circumſtance alſo which ſeems rather to favour this idea is, that, in ſome obſervations, a bright ſpot has been ſeen to project equally on both ſides, as the ſatellites have been obſerved to do when they paſſed behind the ring. But, on the other hand, we ought to conſider that the ſpot has often been obſerved very near the end of the arms of Saturn's ring, and that the calculated diſtance is conſequently a little too ſmall for ſuch appearances, and ought to be 19 or 20 ſeconds at leaſt. We ſhould alſo attend to the ſize of the ſpot, which ſeems to be variable; for it is hardly to be imagined that a ſatellite, brighter than the ſixth, and which could be ſeen with the moon nearly at the full, ſhould ſo often eſcape our notice in its frequent revolutions, unleſs it varied much in its apparent brightneſs.

To this we muſt add another argument drawn from the number of lucid ſpots, which will not agree with the motion of one ſatellite only; whereas, by admitting a revolution of the ring itſelf, in 10 h. 32′ 15″,4; and ſuppoſing all the ſpots to adhere to the ring, and to ſhare in the ſame periodical return, provided they laſt long enough to be ſeen many times,

we

we ſhall be able to give an eaſy ſolution to all the remaining obſervations.

For inſtance, let α, β, γ, δ, ε, repreſent five ſpots on the ring of Saturn, ſituated as in fig. 2.; where the ring is ſuppoſed to be divided into 360 degrees, and the ſpot α placed at 271°,5; β at 70°,2; γ at 183°,0; δ at 142°,5; and ε at 358°,6. Then will the ring, with the ſpots thus placed, ſerve as an epocha for the year 1789; by which, with the aſſiſtance of a table conſtructed upon the before-mentioned period of the rotation of the ring, we may calculate their ſituation for any required time; and to render this calculation perfectly convenient, I have given a table, ready prepared for the purpoſe, at the end of the other tables.

The following obſervations have all been previouſly calculated by the tables of ſuch of the ſeven ſatellites as were not already in view, and have been found to belong to neither of them; but in the notes that are given with them they have been again calculated by the table of the rotation of the ring for every time they were obſerved, on a ſuppoſition of their being ſpots adhering to it.

Obſervations not accounted for by ſatellites.

July 28. 22 31. I now perceive between the neareſt ſat. and ♄, on the ſ. ſide, a ſmall lucid point, like an emerging ſatellite (A).

22 37.

(A) My ſurmiſe of its being an emerging ſatellite ſo early as the beginning of the ſeaſon, when I was ſtill unacquainted with the minute phænomena that offered themſelves afterwards, ſhews plainly, that the lucid point was of a ſufficient brightneſs to deſerve notice. The five old ſatellites were in view, and the

22 37. The laſt diſcovered point, not quite half-way between the 3d ſat. and the body of Saturn; may be it is a 6th ſat. By a figure, the greateſt part of its diameter is to the north of the ring (B).

Aug. 29. 23 1. The preceding projection contains a ſmall inequality. By a figure, it follows the 3d ſat. about $\frac{1}{3}$ of the projection of the ring (C).

Sept. 16. 19 39. I ſuſpect one of the ſatellites cloſe to the ring following (D).

20 6. I am pretty ſure there is a ſatellite cloſe to the following arm, and a very little to the north. 300 leaves it doubtful (E).

Oct. 15. 20 58. I ſuſpect a ſatellite upon the preceding projection, not far from the end of it (F).

21 39. I cannot perceive the ſatellite on the preceding arm ſuſpected at 20 h. 58′ (G).

6th and 7th by calculation could not occaſion this appearance, the former being at 72°,3; the latter on the oppoſite ſide at 299°,4. Suppoſing this, therefore, to be the ſpot I have called α, its place for 13 h. 53′ 39″ would be 36°,8; which might make it appear like an emerging ſatellite.

(B) By this time the ſpot was at 40°,3, which agrees with the obſerved ſituation. As the greateſt part of its diameter appeared to be north, we may ſurmiſe, that the ſpot, which muſt have been of a very conſiderable ſize and brightneſs, was ſituated on the northern plane of the ring, and within a ſecond or two from the outward edge of it. The ring itſelf was now ſo near having its edge directed towards us, that it required no great elevation of the ſpot to render it viſible, notwithſtanding it was then in the fartheſt part of its circuit.

(C) The ſpot α, at 12 h. 17′ 58″ was 301°,5.

(D) The ſpot β, at 7 h. 45′ 52″ was 58°,1. This ſpot was probably alſo on the northern plane, and on the very edge, but not ſo conſiderable as α.

(E) It was now advanced to 73°,4.

(F) The ſpot γ, at 7 h. 10′ 49″ was 305°,0. Its ſituation on the ring was probably on the ſouthern plane, and at ſome conſiderable diſtance from the outward edge.

(G) It was now advanced to 328°,3; and therefore could hardly be ſeen any longer.

Oct. 16. 1 29. I was not without a ſuſpicion of another ſatellite upon the preceding arm, not quite ſo far advanced as the former (H).

Oct. 18. 20 22. I ſuſpect two ſatellites upon the p. arm (I).

20 42. I make no doubt but that there is at leaſt one ſat. upon the p. arm (K).

21 14. I am in doubt whether it be a ſat. upon the p. arm, or the arm itſelf (L).

21 17. Unleſs the p. arm be much brighter than the f. one, it muſt contain a ſatellite (M).

1 1. I am pretty ſure the end of the preceding projection is loaded with two ſatellites. By a figure, one is placed $\frac{1}{4}$ projection from the end; the other, $\frac{1}{3}$ projection from the body (N).

Oct. 18. 1 5. I can diſtinguiſh one upon the preceding projection very certainly (O).

Oct. 20. 21 26. I ſuſpect the end of the preceding arm to be loaded with a ſatellite (P)

(H) The ſpot β. By calculation, at 11 h. 37′ 9″ it was 309°,2, or juſt following the 7th ſatellite, which appeared then upon the preceding arm in the ſhape of a ſmall bright point.

(I) The ſpots γ and δ. The former at 6 h. 23′ 9″ was 217°,6, the latter 289°,9. δ is probably a ſpot upon the northern plane of the ring of a conſiderable degree of brightneſs, though but ſmall in its dimenſions, and at no great diſtance from the edge.

(K) The ſpot γ was by this time at 229°,0, and therefore in a ſituation to be eaſily perceived.

(L) The ſpot γ 247°,2.

(M) The ſpot γ 248°,9.

(N) The ſpot α, at 11 h. 1′ 23″, was at 217°,2; and ε was alſo viſible, being at 304°,3. This ſpot is probably a very ſmall one, on the northern plane of the ring, at ſome diſtance from the edge.

(O) The ſpot α was now at 219°,4, and being very bright could be diſtinguiſhed eaſily.

(P) The ſpot α, at 7 h. 19′ 7″, was at 290°,7.

21 56. The preceding arm is certainly loaded with one or two ſatellites, or is more knotty than I have ever obſerved it to be. The weather is very beautiful (Q).

Oct. 30. 20 53. I ſuppoſe the 7th to be upon the following arm $\frac{1}{4}$ dia. of ♄ f. the body (R).

23 55. The 7th ſat. $\frac{3}{8}$ dia. of ♄ p. the body, or very near to the end of the p. arm, in the ſhape of a protuberant point (S).

0 1. I ſee it ſo well that there is no doubt but that it is a ſatellite (T).

0 42. The 7th is upon the p. arm, but a little nearer than it was before (V).

0 47. The 7th ſat. $\frac{3}{8}$ dia. of ♄ p. the body (W).

Oct. 31. 21 13. The 7th ſat. $\frac{3}{8}$ dia. of ♄ p. the body (X).

21 43. The 7th ſat. is brighter than uſual; I ſee it with great eaſe, notwithſtanding the moon is almoſt at the full. It is brighter now than the 6th (Y).

22 11. The 7th is drawn nearer to the body of ♄. Flying clouds prevent eſtimations of the diſtance (Z).

23 13. The 7th is now no longer viſible (A).

Nov. 2. 22 14. The 7th ſat. $\frac{3}{8}$ dia. of ♄ p. the body, it is upon the arm (B).

(Q) The ſpot α was now at 307°,8; and at the ſame time the ſpot β being come on as far as 219°,3, was therefore viſible.

(R) The ſpot δ, at 6 h. 6′ 57″ was at 40°,3.

(S) The ſpot α, at 9 h. 8′ 28″, was at 272°,4.

(T) It was now at 275°,8.

(V) At 299°,1. (W) At 302°,0.

(X) The ſpot α, at 6 h. 22′ 59″, was 278°,4.

(Y) It was now at 295°,5.

(Z) At 305°,7. (A) At 346°,7.

(B) The ſpot ε, at 7 h. 15′ 58″, was at 235°,6.

22 53. The 7th appears to be full ¼ dia. of ♄ p. the body; but hazy weather (C).

23 13. The 7th ſat. ¼ dia. of ♄ p. the body (D).

0 8. The following arm contains a lucid point near the end of it (E).

0 16. The preceding arm ſeems to be loaded with two ſmall points towards the end (F).

Nov. 4. 22 14. The 7th ſat. I think, is between the 1ſt and 6th, but I cannot be ſure (G).

22 27. I cannot perceive the 7th ſat. where I ſuſpected it (H).

23 47. The f. arm about ¼ dia. of ♄ from the body contains a ſmall lucid point (I).

23 54. I ſee the point on the f. arm ſo well that I have not much doubt but that it is a ſatellite (K).

Nov. 7. 22 9. At the end of the p. arm is a place that is brighter than nearer to the body (L).

23 12. The preceding arm has ſtill the appearance of a ſmall protuberant point towards the ſouth, near the end of the arm (M).

Nov. 8. 23 40. There is a protuberant point on the preceding arm beſides the 7th ſat.; ſo that at preſent I cannot tell whether the ſatellite be the neareſt or fartheſt of them (N).

(C) At 257°,8.

(D) At 269°,1.

(E) The ſpot δ, at 84°,4.

(F) The ſpot ε, at 305°,0.

(G) The ſpot ε, at 7 h. 8′ 7″, was at 71°,0, which agrees with the place, and it might be the ſuppoſed ſatellite.

(H) At 78°,3.

(I) The ſpot α, at 8 h. 40′ 51″, was at 36°,7.

(K) It was now at 40°,7.

(L) The ſpot α, at 6 h. 51′ 21″, was at 274°,0.

(M) It was at 309°,8.

(N) The ſpot δ, at 8 h. 18′ 11″, was at 294°,4.

Nov.

Nov. 10. 21 35. The following arm ¼ dia. of ♄ from the body contains a bright point; perhaps an 8th ſatellite (O).

Nov. 25. 1 21. The p. arm is loaded (P).

Nov. 29. 0 38. There are two ſmall luminous points on the p. arm (Q).

Dec. 5. 0 8. Upon the end of the p. arm appears to be a bright point (R).

0 10. The ſpot on the preceding arm is rather larger than the 6th ſatellite (S).

Dec. 16. 0 7. The end of the p. arm ſeems to be loaded with a ſatellite (T).

Dec. 24. 0 7. The p. arm contains a pretty bright point ⅔ towards the end of it (V).

Dec. 25. 23 39. The p. arm very near the end is loaded with a ſat. (W).

1 10. The p. arm is loaded very nearly at the far end of it, and a little towards the ſouth (X).

1 37. The bright point is near the far end of the p. arm (Y).

The great accordance between the obſerved places of theſe ſpots and the calculated ones, ſeems to eſtabliſh the rotation of the ring of Saturn on an axis ſo as hardly to leave any doubt upon the ſubject. The time of it, we have already ſeen, is 10 hours, 32 minutes, and 15,4 ſeconds. It may be objected,

(O) The ſpot δ, at 6 h. 5′ 40″, was at 59°,1.
(P) The ſpot ε, at 8 h. 52′ 10″, was at 68°,6.
(Q) The ſpot β, at 7 h. 53′ 35″, was at 259°,6.
(R) The ſpot ε, at 7 h. 0′ 7″, was at 283°,8.
(S). It was now at 285°,0.
(T) The ſpot ε, at 6 h. 15′ 57″, was at 277°,4.
(V) The ſpot α, at 5 h. 44′ 33″, was at 251°,6.
(W) The ſpot β, at 5 h. 12′ 42″, was at 244°,8.
(X) It was now at 296°,0. (Y) And now at 311°,4.

that

that many of the obſervations are ſuch as would alſo agree with other aſſignable periods, eſpecially when the numbers of ſpots is ſo conſiderable as five; but the moſt material obſervations, which are thoſe on the ſpot α, ſetting aſide all the reſt, ſeem alone to amount to a proof not only of a rotation of the ring, but of the time in which it is performed.

It may be expected, that having now ſufficiently examined the whole ſeries of obſervations of the laſt new ſatellites, we can give their periodical times and diſtances more accurately than before. The times, indeed, are full as well aſcertained as we can expect to have them: for on calculating ſix ſatellites by my tables back to Aug. 19 d. 12 h. 19′ 56″, 1787, we find their places 341°,1 the 5th; 10°,6 the 4th; 211°,1 the 3d; 158°,9 the 2d; 80°,2 the 1ſt; and 288°,8 the 6th. And my journal contains the fulleſt aſſurance that they were thus ſituated at the time for which this calculation is made. We may therefore fix the period of the ſixth at 1 d. 8 h. 53′ 8″,9. The 7th ſatellite can only be traced back as far as the 8th of Sept. 1789; ſo that its revolution will require at leaſt another ſeaſon to come to ſome degree of accuracy, till when we ſhall ſtate it at 22 h. 37′ 22″,9.

The diſtance of theſe ſatellites, deduced from calculation, depends intirely upon the time and diſtance of the 4th, which is the ſatellite that has been uſed. In order to obtain more accuracy in theſe elements, I have applied myſelf to meaſuring the diſtance of the 4th ſatellite in thoſe moments which were moſt favourable for the purpoſe. It is well known that this ſubject, on account of the quantity of matter in Saturn, to be deduced from the periodical times and diſtances of the ſatellites, is of conſiderable importance to aſtronomers; I ſhall

therefore defer a full inveſtigation of it till I can have an opportunity of calculating a great number of meaſures, not only of the 4th and 5th, but alſo of the other ſatellites which I have already by me, and ſtill intend next ſeaſon to take. Mean while, having brought the meaſures of the 30th of November, which ſeem to me to be very good ones, to the mean diſtance of Saturn from the ſun, I find they give the diſtance of the 4th ſatellite from Saturn 3′ 8″,918. In reducing theſe meaſures to the mean diſtance, I have uſed the new tables of M. DE LAMBRE for Saturn, and MAYER's for the ſun.

Admitting therefore the above quantity as the diſtance, and 15 d. 22 h. 41′ 13″,4 as the period of the 4th ſatellite, we compute that the diſtance of the 6th from the center of Saturn is 36″,7889; and that of the 7th, 28″,6689.

Tables for the ſeven ſatellites of Saturn.

Epochs of the mean longitude of the ſatellites.

	5. ſat.	4. ſat.	3. ſat.	2. ſat.	1. ſat.	6. ſat.	7. ſat.
Years.	Deg.dec	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.
1787	335,91	149,16	87,21	272,18	176,46	269,31	307,07
1788	196,84	132,41	93,86	173,95	131,91	307,48	65,02
1789	53,23	93,09	20,82	304,19	256,66	82,92	161,00
1790	269,63	53,77	307,78	74,43	21,41	218,36	256,98
1791	126,02	14,45	234,74	204,68	146,16	353,81	352,97

Saturnicentric

Saturnicentric motion of the ſatellites in months.

	5th.	4th.	3d.	2d.	1ſt.	6th.	7th.
Months	Deg.dec	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.
January	000,00	000,00	000,00	000,00	000,00	000,00	000,00
Februa.	140,68	339,89	310,40	117,58	151,64	224,54	320,81
March	267,75	252,05	21,73	200,56	91,18	20,91	215,73
April	48,43	231,95	332,13	318,14	242,81	245,45	176,54
May	184,57	189,26	202,84	304,19	203,75	207,27	115,39
June	325,25	169	153,24	61,77	355,39	71,81	76,20
July	101,39	126,47	23,94	47,82	316,33	33,63	15,05
Auguſt	242,07	106,37	334,34	165,40	107,96	258,17	335,86
Septem.	22,75	86,26	284,74	282,98	259,60	122,72	296,67
October	158,89	43 58	155,45	269,03	220,54	84,54	235,52
Novem.	299,57	23,47	105,85	26,61	12,17	309,08	196,33
Decem.	75,71	340,78	336,56	12,66	333,11	270,90	135,17

In the months January and February of a biſſextile year ſubtract 1 from the number of days given.

Motion of the ſatellites in days.

	5th.	4th.	3d.	2d.	1ſt.	6th.	7th.
Days	Deg. Dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.
1	4,54	22,58	79,69	131,53	190,70	262,73	21,96
2	9,08	45,15	159,38	263,07	21,40	165,45	43,92
3	13,61	67,73	239,07	34,60	212,09	68,18	65,88
4	18,15	90,31	318,76	166,14	42,79	330,91	87,85
5	22,69	112,89	38,45	297,67	233 49	233,64	109,81
6	27,23	135,46	118,14	69,21	64,19	136,36	131,77
7	31,77	158,04	197,83	200,74	254,89	39,09	153,73
8	36,30	180,62	277,52	332,28	85,58	301,82	175,69
9	40,84	203,19	357,21	103,81	276,28	204,55	197,65
10	45,38	225,77	76,90	235,35	106,98	107,27	219,62
11	49,92	248,35	156,59	6,88	297,68	10,00	241,58
12	54,46	270,93	236,28	138,42	128,38	272,73	263,54
13	58,99	293,50	315,97	269,95	319,07	175,45	285,50
14	63,53	316,08	35,66	41,49	149,77	78,18	307,46
15	68,07	338,66	115,35	173,02	340,47	340,91	329,42
16	72,61	1,24	195,04	304,56	171,17	243,64	351,39
17	77,15	23,81	274,74	76,09	1,87	146,36	13,35
18	81,69	46,39	354,43	207,63	192,56	49,09	35,31
19	86,22	68,97	74,12	339,16	23,26	311,82	57,27
20	90,76	91,54	153,81	110,70	213,96	214,54	79,23
21	95,30	114,12	233,50	242,23	44,66	117,27	101,19
22	99,84	136,70	313,19	13,77	235,35	20,00	123,16
23	104,38	159,28	32,88	145,30	66,05	282,73	145,12
24	108,91	181,85	112,57	276,84	256,75	185,45	167,08
25	113,45	204,43	192,26	48,37	87,45	88,18	189,04
26	117,99	227,01	271,95	179,91	278,15	350,91	211,00
27	122,53	249,58	351,64	311,44	108,84	253,64	232,96
28	127,07	272,16	71,33	82,98	299,54	156,36	254,92
29	131,60	294.74	151,02	214,51	130,24	59,09	276,89
30	136,14	317,32	230,71	346,05	320,94	321,82	298,85
31	140,68	339,89	310,40	117,58	151,64	224,54	320,81

Motion of the ſatellites in hours.

	5th.	4th.	3d.	2d.	1ſt.	6th.	7th.
Hours.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.
1	0,19	0,94	3,32	5,48	7,95	10,95	15,92
2	0,38	1,88	6,64	10,96	15,89	21,89	31,83
3	0,57	2,82	9 96	16,44	23,84	32,84	47,75
4	0,76	3,76	13,28	21,92	31,78	43,79	63.66
5	0,95	4,70	16,60	27,40	39,73	54,73	79,58
6	1,13	5,64	19,92	32,88	47,67	65,68	95,49
7	1,32	6,58	23,24	38,36	55,62	76,63	111,41
8	1,51	7,53	26,56	43,84	63,57	87,58	127,32
9	1,70	8,47	29,88	49,33	71,51	98,52	143,24
10	1,89	9,41	33,20	54,81	79,46	109,47	159,15
11	2,08	10,35	36,52	60,29	87,40	120,42	175,07
12	2,27	11,29	39,84	65,77	95,35	131,36	190,98
13	2,46	12,23	43,17	71,25	103,29	142,31	206,90
14	2,65	13,17	46,49	76,73	111,24	153,26	222,81
15	2,84	14,11	49,81	82,21	119,19	164,20	238,73
16	3,03	15,05	53,13	87,69	127,13	175,15	254,64
17	3,21	15,99	56,45	93,17	135,08	186,10	270,56
18	3,40	16,93	59,77	98,65	143,02	197,05	286,47
19	3,59	17,87	63,09	104,13	150,97	207,99	302,39
20	3,78	18,81	66,41	109,61	158,91	218,94	318,30
21	3,97	19,75	69,73	115,09	166,86	229,89	334,22
22	4,16	20,70	73,05	120,57	174,81	240,83	350,13
23	4,35	21,64	76,37	126,05	182,75	251,78	6,05
24	4,54	22,58	79,69	131,53	190,70	262,73	21,96

Motion of the ſatellites in minutes.

	5th.	4th.	3d.	2d.	1ſt.	6th.	7th.
Min.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.
1	0,00	0,02	0,06	0,09	0,13	0,18	0,27
2	0,01	0,03	0,11	0,18	0,26	0,36	0,53
3	0,01	0,05	0,17	0,27	0,40	0,55	0,80
4	0,01	0,06	0,22	0,37	0,53	0,73	1,06
5	0,02	0,08	0,28	0,46	0,66	0,91	1,33
6	0,02	0,09	0,33	0,55	0,79	1,09	1,59
7	0,02	0,11	0,39	0,64	0,93	1,28	1,86
8	0,03	0,13	0,44	0,73	1,06	1,46	2,12
9	0,03	0,14	0,50	0,82	1,19	1,64	2,39
10	0,03	0,16	0,55	0,91	1,32	1,82	2,65
11	0,04	0,17	0,61	1,00	1,46	2,01	2,92
12	0,04	0,19	0,66	1,10	1,59	2,19	3,18
13	0,04	0,20	0,72	1,19	1,72	2,37	3,45
14	0,05	0,22	0,77	1,28	1,85	2,55	3,71
15	0,05	0,24	0,83	1,37	1,99	2,74	3,98
16	0,05	0,25	0,89	1,46	2,12	2,92	4,24
17	0,06	0,27	0,94	1,55	2,25	3,10	4,51
18	0,06	0,28	1,00	1,64	2,38	3,28	4,78
19	0,06	0,30	1,05	1,73	2,52	3,47	5,04
20	0,07	0,31	1,11	1,83	2,65	3,65	5,31
21	0,07	0,33	1,16	1,92	2,78	3,83	5,57
22	0,07	0,34	1,22	2,01	2,91	4,01	5,84
23	0,08	0,36	1,27	2,10	3,05	4,20	6,10
24	0,08	0,38	1,33	2,19	3,18	4,38	6,37
25	0,08	0,39	1,38	2,28	3,31	4,56	6,63
26	0,09	0,41	1,44	2,37	3,44	4,74	6,90
27	0,09	0,42	1,49	2,47	3,57	4,93	7,16
28	0,09	0,44	1,55	2,56	3,71	5,11	7,43
29	0,10	0,45	1,60	2,65	3,84	5,29	7,69
30	0,10	0,47	1,66	2,74	3,97	5,47	7,96

Motion of the satellites in minutes.

	5th	4th.	3d	2d.	1st.	6th.	7th.
Min.	Deg. Dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.	Deg. dec.
31	0,10	0,49	1,72	2,83	4,10	5,66	8,22
32	0,11	0,50	1,77	2,92	4,24	5,84	8,49
33	0,11	0,52	1,83	3,01	4,37	6,02	8,75
34	0,11	0,53	1,88	3,10	4,50	6,20	9,02
35	0,12	0,55	1,94	3,20	4,63	6,39	9,29
36	0,12	0,56	1,99	3,29	4,77	6,57	9,55
37	0,12	0,58	2,05	3,38	4,90	6,75	9,82
38	0,13	0,60	2,10	3,47	5,03	6,93	10,08
39	0,13	0,61	2,16	3,56	5,16	7,12	10,35
40	0,13	0,63	2,21	3,65	5,30	7,30	10,61
41	0,14	0,64	2,27	3,74	5,43	7,48	10,88
42	0,14	0,66	2,32	3,83	5,56	7,66	11,14
43	0,14	0,67	2,38	3,93	5,69	7,85	11,41
44	0,15	0,69	2,43	4,02	5,83	8,03	11,67
45	0,15	0,71	2,49	4,11	5,96	8,21	11,94
46	0,15	0,72	2,55	4,20	6,09	8,39	12,20
47	0,16	0,74	2,60	4,29	6,22	8,58	12,47
48	0,16	0,75	2,66	4,38	6,36	8,76	12,73
49	0,16	0,77	2,71	4,47	6,49	8,94	13,00
50	0,17	0,78	2,77	4,57	6,62	9,12	13,27
51	0,17	0,80	2,82	4,66	6,75	9,30	13,53
52	0,17	0,82	2,88	4,75	6,88	9,49	13,80
53	0,17	0,83	2,93	4,84	7,02	9,67	14,06
54	0,18	0,85	2,99	4,93	7,15	9,85	14,33
55	0,18	0,86	3,04	5,02	7,28	10,03	14,59
56	0,18	0,88	3,10	5,11	7,41	10,22	14,86
57	0,19	0,89	3,15	5,20	7,55	10,40	15,12
58	0,19	0,91	3,21	5,30	7,68	10,58	15,39
59	0,19	0,93	3,27	5,39	7,81	10,76	15,65
60	0,20	0,94	3,32	5,48	7,94	10,95	15,92

Table of the rotation of the ring of Saturn.

Epochs for 1789.		Motion of the spots in days, hours, and minutes.							
		Days	Deg.dec	Hou.	Deg.dec	Min.	Deg.dec	Min.	Deg.dec
Spot α	271,5	1	99,92	1	34,16	1	0,57	31	17,65
β	183,0	2	199,84	2	68,33	2	1,14	32	18,22
γ	70,2	3	299,76	3	102,49	3	1,71	33	18,79
δ	142,5	4	39,68	4	136,65	4	2,28	34	19,36
ε	358,6	5	139,60	5	170,81	5	2,85	35	19,93
		6	239,52	6	204,98	6	3,42	36	20,50
Motion of the spots in Months.		7	339,44	7	239,14	7	3,99	37	21,07
		8	79,36	8	273,30	8	4,56	38	21,64
Months.	Deg. dec.	9	179,28	9	307,46	9	5,12	39	22,21
January	000,00	10	279,20	10	341,63	10	5,69	40	22,78
February	217,52	11	19,12	11	15,79	11	6,26	41	23,35
March	135,28	12	119,04	12	49,95	12	6,83	42	23,91
April	352,80	13	218,96	13	84,11	13	7,40	43	24,48
May	110,40	14	318,88	14	118,28	14	7,97	44	25,05
June	327,92	15	58,80	15	152,44	15	8,54	45	25,62
July	85,52	16	158,72	16	186,60	16	9,11	46	26,19
Auguſt	303,04	17	258,64	17	220,76	17	9,68	47	26,76
September	160,56	18	358,56	18	254,93	18	10,25	48	27,33
October	278,16	19	98,48	19	289,09	19	10,82	49	27,90
November	135,68	20	198,40	20	323,25	20	11,39	50	28,47
December	253,28	21	298,32	21	357,41	21	11,96	51	29,04
		22	38,24	22	31,59	22	12,53	52	29,61
		23	138,16	23	65,75	23	13,10	53	30,18
		24	238,08	24	99,91	24	13,67	54	30,75
		25	338,00			25	14,24	55	31,32
		26	77,92			26	14,80	56	31,89
		27	177,84			27	15,37	57	32,46
		28	277,76			28	15,94	58	33,03
		29	17,68			29	16,51	59	33,59
		30	117,60			30	17,08	60	34,16
		31	217,52						

Example

South

360 10 20 30 40 50 60 70 80 90 100 110 120 130 140 150 160 170 180

Following

North

The seven Satellites of Saturn as they appeared to be situated the 18.th of Oct.r at 7.h 51'. 54". 1789.

1.st Sat.

2.d Sat.

7.th Sat.

or North

360 350 340 330 320 310 300 290 280 270 260 250 240 230 220 210 200 190 180

Preceding

South

5th Satellite

3d Satellite

4th Satellite

1st Sat.

6th Sat.

7th Sat.

Fig. 1.

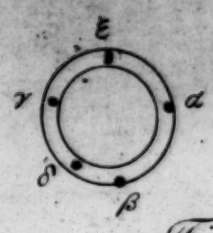


Fig. 2.

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Example of the use of the tables.

Let it be required to calculate the apparent place of the seven satellites for 1789, Oct. 18. 7 h. 51′ 54″, to the nearest minute of time and to tenths of a degree.

	5th.	4th.	3d.	2d.	1st.	6th.	7th.
1789	53,23	93,09	20,82	304,19	256,66	82,92	161,00
Oct.	158,89	43,58	155,45	269,03	220,54	84,54	235,52
18	81,69	46,39	354,43	207,63	192,56	49,09	35,31
7	1,32	6,58	23,24	38,36	55,62	76,63	111,41
52	0,17	0,82	2,88	4,75	6,88	9,49	13,80
* ♄	12,58	12,58	12,58	12,58	12,58	12,58	12,58
	307,9	203,0	209,4	116,5	24,8	315,3	209,6

The situation of the spot α calculated for July 28. 13 h. 53′ 39″; β for Sept. 16. 7 h. 45′ 48″; ε for Nov. 2. 7 h. 15′ 58″.

1789, α	271,5	β	183,0	ε	358,6
July	85,52	Sept.	160,56	Nov.	135,68
28	277,76	16	158,72	2	199,84
13	84,11	7	239,14	7	239,14
54	30,75	46	26,19	16	9,11
♄	7,20	♄	10,45	♄	13,18
	36,8		58,1		235,6

* The quantity marked ♄ 12°,58, which is applied to every one of the satellites, is the complement of 11^s 17° 25′, or geocentric place of Saturn, taken from the Nautical Almanac, for midnight of the required day, and to the nearest minute, which is sufficiently exact. This complement, or 12° 35′ in conformity with the tables, is reduced to decimals of a degree 12°,58.

XXIV. *On Spherical Motion.* *By the Rev.* Charles Wildbore; *communicated by Earl* Stanhope, *F. R. S.*

Read June 24, 1790.

THIS Paper, which has coſt me much pains in patient inveſtigation, is occaſioned by that of Mr. LANDEN, in the Philoſophical Tranſactions, Vol. LXXV. Part II. I am no ſtranger to this gentleman's great judgement and abilities in theſe abſtruſe ſpeculations, but have a very high opinion of both; yet I could not but think it ſtrange, that two ſuch mathematicians as M. D'ALEMBERT and M. L. EULER ſhould both follow one another on the ſame ſubject, both agree, and ſtill not be right. I therefore reſolved to try to dive to the bottom of their ſolutions, which thoſe who are acquainted with the ſubject know to be no light taſk; and, if poſſible, to give the ſolution, independent of the perplexing conſideration of a momentary axis changing its place both in the body and in abſolute ſpace every inſtant; and which I look upon as not abſolutely eſſential to the determination of the body's motion. But finding that I could not thus ſo readily ſhew the agreement or diſagreement of my concluſions with thoſe of the gentlemen who have preceded me in this enquiry; I have alſo added the inveſtigation of the properties of this axis. And I ſuppoſe it will be found, that I have added many properties unknown before, or at leaſt unnoticed by any of them.

M. LANDEN's

M. LANDEN's very important diſcovery, that every body, be its form ever ſo irregular, will revolve in the ſame manner as if its maſs were equally divided and placed in the eight angles, or diſpoſed in the eight octants of a regular parallelopipedon, whoſe moments of *inertia* round its three permanent axes are the ſame as thoſe of the body, ſerves admirably to ſhorten the inveſtigation, and render the ſolution perſpicuous. I have therefore here taken its truth for granted, becauſe it is alſo exactly agreeable to the ſolutions of the other gentlemen, and ſaves the trouble of repeating what they have done before. I have alſo ſhewn wherein, and why, his ſolution differs from theirs, and proved, as I think, undeniably, in what reſpects it is defective.

That the *inertia*, or, as M. EULER calls it, the *momentum* of *inertia*, is equal to the fluent or ſum of every particle of the body drawn into the ſquare of its diſtance from the axis of motion; and the determination of the three permanent axes, or the demonſtration that there are, at leaſt, three ſuch axes in every body, round any one of which, if it revolved, the velocity would be for ever uniform, I have alſo taken for granted, becauſe theſe things have been proved before, and all the gentlemen are agreed in them. Difficulties that occurred I have not concealed, but ſhewn how to obviate, and endeavoured to place the truth in as clear a light as poſſible; which to diſcover is my wiſh, or to welcome it by whomſoever found.

PROPOSITION I.

Whilſt a globe, whoſe centre is at reſt, revolves with a given velocity about an axis paſſing through that centre, to

find with what velocity any great circle on the ſurface, but oblique to that axis, moves along itſelf.

Let I (Tab. XX. fig. 1.) be the centre, and BL*b* the axis round which the globe revolves with a velocity = *c* meaſured along the great circle GH, whoſe plane is perpendicular to that axis, and HSG*s* any great circle whoſe plane is oblique to the axis, ESF and *esf* two leſſer circles of the ſphere parallel to the great circle GH, and touching HSG*s* in S and *s*; then, as the radius BI which may be ſuppoſed unity : *c* :: the radius of the leſs circle ESF = the ſine of the arc BE or BS : the velocity along the circle ESF = the abſolute velocity of the point S on the ſurface of the globe: but the point S is alſo upon the great circle GSH*s*, and therefore this is alſo equal to the velocity of the point *s* along the great circle GSH*s*; and for the ſame reaſons the point S, which is diametrically oppoſite to S on the ſurface, has alſo the ſame velocity. Let P be any other point in the great circle GSH*s*; then, ſince as the globe revolves the diſtances SP and *s*P always continue invariable, the velocity of the point P in the circle HPS in the direction of the periphery of the circle itſelf muſt be equal to that of S and *s*; and is therefore the velocity of every point of this circle along its own periphery.

Corollary 1. Hence it follows, that in whatſoever manner a globe revolves, its velocity meaſured on the ſame great circle on its ſurface muſt be the ſame at the ſame time at every point of the periphery of that circle.

Corollary 2. Conſequently, howſoever the plane of a great circle varies its motion, the velocity at any inſtant is at every point of the periphery equal along its own plane.

DEFI-

DEFINITION.

The points S and *s*, where a great circle from the poles B and *b* of the natural axis cuts any great circle GSH*s* (at right angles) I call the nodes of that great circle.

Corollary 3. If O be the pole of the great circle HSG*s*, then the globe may be conſidered as moving round the axis whoſe pole is O with a velocity $= c \times \frac{\text{ſine of BS}}{\text{BI}}$, whilſt the pole O is carried along the leſſer circle AOA, which is parallel to the midcircle GH with a velocity $= c \times \frac{\text{ſin. O}b}{\text{BI}} = c \times \frac{\text{coſ. BS}}{\text{BI}}$; and this way of conſidering the motion, which is uſeful in what follows, comes to the very ſame as the motion along the great or midcircle GH with the velocity $= c$, becauſe $c^2 \times \frac{\text{ſ. BS}^2}{\text{BI}^2} + c^2 \times \frac{\text{coſ. BS}^2}{\text{BI}^2} = c^2$. Conſequently, the ſum of the ſquares of the velocities at the node and pole of any great circle upon a ſpherical ſurface thus revolving, is equal to the ſquare of the velocity round the natural (or momentary) axis BI*b*.

Corollary 4. Since the pole O is at 90° diſtance from the node S, its motion can have no effect at S or *s*, the motion at the nodes, therefore, of the great circle HSG*s* is that of the great circle along its own proper plane; but any other point, as P, partakes both of the motion along the circle, and the motion of its pole. The direction of its motion being along the leſſer circle P*p*, parallel to FSE, and its velocity therein $= c \times \frac{\text{ſ. BP}}{\text{ſ. BS}}$; the velocity of P therefore, in the direction of the great circle OP, which is perpendicular to SP in P, is $= c\sqrt{\left(\frac{\text{ſ. BP}^2}{\text{ſ. BS}^2} - \frac{\text{ſ. BS}^2}{\text{BI}^2}\right)}$, and along the great circle BP its velocity $= 0$.

PROPOSITION II.

Suppoſing the centre of a ſphere to be at reſt, whilſt the ſurface moves round it in any manner whatſoever; then, if the ſame invariable point O, conſidered as the pole of an axis of the ſphere, be itſelf in motion, the angular velocity of the ſpherical ſurface about that axis will be unequable, or that of one point therein different from that of another.

For, let I (fig. 2.) be the centre of the ſphere; draw the great circle POF perpendicular to the direction of the motion of the ſurface at O; then muſt the pole of this motion neceſſarily be in ſome point P of this great circle POF. Let FC be the great circle whoſe pole is P, and LQ that whoſe pole is O; then, the velocity of any point F of the great circle FC muſt, by the preceding propoſition, be equal to that of any other point H thereof. Let that velocity be repreſented by the equal arches FG and HK, and from the pole O draw the great circles OGM, OHN, and OKA, cutting the great circle LQ in M, N, and A; then muſt LM repreſent the angular velocity of the point F about the axis IO, and NA that of the point H. But, by Prop. 9. Lib. III. THEODOSII Sphericorum, LM is greater than NA; and conſequently the angular velocity of the point F about IO is greater than that of H; and conſequently the angular velocity of the ſurface about the axis IO is unequable.

Corollary. Hence, about whatever axis the angular motion of a ſphere is equable, the pole of that axis, and conſequently the axis itſelf, muſt be at reſt at the inſtant. Different motions may have different correſpondent poles, and conſequently, when the motion is variable, the place of the pole of equable

motion

motion on the ſurface may vary; but whatever point on the ſurface correſponds with that pole muſt at the inſtant be at reſt.

PROPOSITION III.

Let ABC (fig. 3.) be an octant of a ſpherical ſurface in motion, while the centre is at reſt; and let the velocity of the great circle BC in its own plane $= a$, and in a ſenſe from B towards C; that of CA in the ſenſe from C towards A $= b$, and of AB from A towards B $= c$. If theſe three velocities a, b, and c, be conſtant, the ſpherical ſurface will always revolve uniformly about the ſame axis of the ſphere at reſt in abſolute ſpace.

For, let ABC, *abc*, be two poſitions of the revolving octant indefinitely near each other, A*a*, B*b*, and C*c*, the tracks of A, B, and C, in abſolute ſpace. Perpendicular to A*a* draw the great circle SOA, and perpendicular to B*b* the great circle BOQ, cutting SOA in O and CA in Q; then, becauſe A*a* is indefinitely ſmall, the two triangles A*pa* right-angled at *p*, and *a*′A*q* right-angled at A may be conſidered as plane ones, and are therefore ſimilar; and ſince the angles *p*AQ and *q*A*a* are both right ones, taking away *q*A*p*, which is common, the angles *p*A*a*, *q*AQ, muſt be equal; but as *p*A : *pa* :: *c* : *b*, likewiſe *p*A : *pa* :: ſ. *pa*A : ſ. *p*A*a*, and *pa*A = *p*A*q*, *p*A*a* = *q*AQ; conſequently, as ſ. *p*A*q* : ſ. *q*AQ :: *c* : *b*, that is, the ſines of the angles BAS and CAS are proportional to the velocities along AB and CA; conſequently, the ſines of the arches SB and SC which are the meaſures of thoſe angles muſt be in the ſame ratio. In like manner it appears, that as ſ. CQ : ſ. AQ ::

$a : c$ :: f. CBQ : f. ABQ. Moreover, f. SOB : radius :: f. SB : f. BO :: f. AQ : f. AO. Through C and O draw the great circle COR; then, as f. AO : radius :: f. OAR : f. OR :: f. OAQ : f. OQ, or f. OR : f. OQ :: f. OAR : f. OAQ :: $c : b$, and for a like reaſon, f. OR : f. OS :: f. OBR : f. OBS :: $c : a$, that is, f. OR : c :: f. OQ : b :: f. OS : a, or f. OQ : f. OS :: $b : a$; but f. OQ : f. OS :: f. OCQ = f. AR : f. OCS = f. BR, or f. AR : f. BR :: $b : a$. Now, bc is ultimately perpendicular to AC in d, ſo the triangle Cdc being right-angled at d, the ſum of the angles Ccd, cCd muſt be = a right one, and their ſines are in the ratio of $Cd : cd$, or of $b : a$; but the ſum of the angles OCQ, OCS, is alſo a right one, and their ſines alſo have been proved to be in the ſame ratio of $b : a$, conſequently the angle OCQ = Ccd, and OCS = cCd, to OCS and cCd add the common angle OCQ, and the angle OCc muſt be = BCQ a right one: conſequently OC is perpendicular to Cc the track of the point C, as OA is, by hypotheſis, to Aa, and OB to Bb. The ſines of SO, QO, and RO, are as a, b, and c, alſo f. SO^2 + f. QO^2 + f. RO^2 by trigonometry = the ſquare of radius = 1; hence f. SO^2 + f. QO^2 = 1 − f. RO^2 = f. CO^2; f. SO^2 + f. RO^2 = f. BO^2, f. QO^2 + f. RO^2 = f. AO^2; conſequently, f. AO^2, f. BO^2 and f. CO^2 are as $b^2 + c^2$, $a^2 + c^2$, and $a^2 + b^2$, or as Aa^2, Bb^2, and Cc^2; wherefore the velocities $\sqrt{b^2 + c^2}$, $\sqrt{a^2 + c^2}$, and $\sqrt{a^2 + b^2}$, of the points A, B, C, are in directions perpendicular to AO, BO, and CO, and in the ratio of the ſines of the arches AO, BO, and CO, that is of the diſtances of the points A, B, and C, from the axis whoſe pole is O, the tracks of theſe points are therefore circles of the ſphere whoſe radii are thoſe diſtances. And ſo long as the velocities a, b, and c, are invariable, the points Q, R, and S, which are always at the ſame diſtances

from B, C, and A, muſt be always at the ſame diſtances from O, that is, OR, OS, and OQ, are conſtant, and the point O at reſt. And this muſt alſo be the caſe if a, b, and c be variable, provided they have the ſame conſtant ratio amongſt themſelves.

Corollary. Hence the points Q, R, and S, are the nodes of the great circles CQA, ARB, and BSC.

Scholium. The demonſtration of this propoſition being thus ſtrictly given, ſome notion may be obtained of the manner in which the point O varies its place upon the ſpherical ſurface when the velocities along the circles AB, BC, and CA, are variable. Thus, let ſuch ſpherical ſurface, ſo revolving, receive an inſtantaneous impulſe, at the diſtance of a quadrant or 90° from S, in a direction perpendicular to the plane of the great circle CSB; then, the centre of the ſphere may be kept at reſt by an equal and contrary impulſe at this centre; and ſince, by hypotheſis, the impulſe is given 90° from S, and in a direction perpendicular to the plane of the great circle CSB, it can neither alter the place of the node S upon the circle, nor the velocity in the direction of its periphery, but only thoſe in AB and CA. Thus, if the velocity in BA which before was $= c$ be now equal to z; then, as ſ. SB : z :: ſ. SC : the velocity along CA, let this $= y$, whilſt ſtill the velocity along CB continues as before $= a$; and this will cauſe the point O to fall upon another point of the great circle SA: ſo that whereas before the ſines of OS, OR, and OQ, were as a, c, and b, they ſhall now be as a, z, and y. Conſequently, as ſ. SO : rad. $= 1$ :: a : the velocity at 90° from O, ſ. OR : 1 :: z : the velocity at 90° from O, and ſ. OQ : y :: 1 : velocity at 90° from O, which three quantities muſt therefore be equal to one another, and to the angular velocity of the ſphere about the axis whoſe pole

is

is O; let this angular velocity $=e$, then muſt $e \times$ ſ. SO $=a$, $e \times$ ſ. OR $=z$, and $e \times$ ſ. OQ $=y$, and the ſum of the ſquares of theſe three, or $a^2+z^2+y^2=e^2 \times$ ſ. $SO^2+e^2 \times$ ſ. $RO^2+e^2 \times$ ſ. $QO^2=e^2$, becauſe ſ. SO^2+ſ. RO^2+ſ. $QO^2=1$, hence $e=\sqrt{a^2+z^2+y^2}$; whereas, before the impulſe $e=\sqrt{a^2+b^2+c^2}$. Thus not only the place of O, but, if z^2+y^2 be not $=b^2+c^2$, the angular velocity of the ſphere about its ſingle axis will alſo be altered. Hence then if, inſtead of an inſtantaneous impulſe, a motive force be ſuppoſed to act in the ſame direction, and meaſured at the ſame point where the impulſe was juſt now ſuppoſed to act; ſuch force can neither vary the point S nor the velocity a, but will in time vary b and c, and cauſe the point O to alter its place in SA; and thus the velocities b and c will vary to y and z, and $e=\sqrt{a^2+b^2+c^2}$ to $e=\sqrt{a^2+y^2+z^2}$, juſt as it would have been by a ſingle impulſe, excepting that then, when the impulſe was over, y and z muſt have become conſtant quantities, whereas now they will vary perpetually during the time that the motive force acts, and the point O will ſhift its place ſo as at different times to coincide with different points of AS, though at any one inſtant the point of the ſurface that coincides with it muſt be at reſt, by Prop. 2.

PROPOSITION IV.

If a ſpherical ſurface, whoſe center is at reſt, revolve in any manner whatſoever, ſo that the velocities along the three quadrants bounding any octant thereof be expreſſed by any three variable quantities x, y, and z; to find the neceſſarily correſponding accelerating forces with which the place of the natural

natural or momentary axis, and the angular velocity of the ſurface round it are varied.

Here, other things remaining as in the preceding propoſition, inſtead of the conſtant quantities a, b, and c, we have the variable ones x, y, and z. Let the variable ſines and coſines of AO, BO, and CO, be reſpectively expreſſed by b and β, g and γ, and d and δ; and let t = the time from the commencement of the motion; then it is well known, that the reſpective accelerating forces along CB, CA, and AB, muſt be expreſſed by $\frac{\dot{x}}{\dot{t}}$, $\frac{\dot{y}}{\dot{t}}$, and $\frac{\dot{z}}{\dot{t}}$; and the radius of the ſphere being ſuppoſed = unity, the angular velocity about the axis whoſe pole is $O = e = \sqrt{x^2+y^2+z^2} = e\sqrt{\beta^2+\gamma^2+\delta^2}$, $e\beta = x$, $e\gamma = y$, $e\delta = z$, $\dot{x} = e\dot{\beta}+\beta\dot{e}$, $\dot{y} = e\dot{\gamma}+\gamma\dot{e}$, $\dot{z} = e\dot{\delta}+\delta\dot{e}$, $\beta^2+\gamma^2+\delta^2 = 1$, $\beta\dot{\beta}+\gamma\dot{\gamma}+\delta\dot{\delta} = 0$, $\beta^2+\gamma^2 = 1-\delta^2 = d^2$, $\beta^2+\delta^2 = 1-\gamma^2 = g^2$, $\gamma^2+\delta^2 = 1-\beta^2 = b^2$, $\dot{e} = \frac{x\dot{x}+y\dot{y}+z\dot{z}}{\sqrt{x^2+y^2+z^2}} = \beta\dot{x}+\gamma\dot{y}+\delta\dot{z} = \frac{\dot{x}-e\dot{\beta}}{\beta} = \frac{\dot{y}-e\dot{\gamma}}{\gamma} = \frac{\dot{z}-e\dot{\delta}}{\delta}$. And, by ſpherics, as $g : 1 :: \delta : \frac{\delta}{g}$ = ſ. OBR = ſ. QA :: $\beta : \frac{\beta}{g}$ = ſ. OBS = ſ. CQ = coſ. AQ, tang. AQ = $\frac{\delta}{\beta}$ and the fluxion of the arc AQ = $\frac{\beta\dot{\delta}-\delta\dot{\beta}}{\beta^2+\delta^2}$. But, by the foregoing propoſition, BO is perpendicular to Bb the track of the point B; conſequently, as ſ. OBR = ſ. AQ : ſ. OBS = coſ. AQ :: $z : x$; therefore the tangent of AQ = $\frac{z}{x}$ and $\dot{A}Q = \frac{x\dot{z}-z\dot{x}}{x^2+z^2} = \frac{e\beta\times\overline{e\dot{\delta}+\delta\dot{e}}-e\delta\times\overline{e\dot{\beta}+\dot{\beta}e}}{e^2\beta^2+e^2\delta^2} = \frac{\beta\dot{\delta}-\delta\dot{\beta}}{\beta^2+\delta^2}$ as before; therefore, whether e be conſtant or variable it makes no difference in the expreſſion for $\dot{A}Q$. In like manner it will appear, that $\dot{BR} = \frac{y\dot{x}-x\dot{y}}{x^2+y^2} = \frac{\gamma\dot{\beta}-\beta\dot{\gamma}}{\beta^2+\gamma^2}$, and $\dot{CS} = \frac{z\dot{y}-y\dot{z}}{y^2+z^2} = \frac{\delta\dot{\gamma}-\gamma\dot{\delta}}{\gamma^2+\delta^2}$. Moreover, as

rad. = 1 : the alteration of the place of Q round B, or in the great circle $AC = A\dot{Q} :: f.\ BO = g = \sqrt{\beta^2 + \delta^2} : \frac{\beta\dot{\delta} - \delta\dot{\beta}}{\sqrt{\beta^2 + \delta^2}}$ = the momentary alteration of place of O round B, or in a direction perpendicular to the great circle BOQ at O, and the corresponding alteration of BO, that is, $B\dot{O} = -\frac{\dot{\gamma}}{\sqrt{\beta^2 + \delta^2}} = -\frac{\dot{\gamma}}{g}$, the fluxion therefore of the track of O upon the ſpherical ſurface $= \sqrt{\frac{\beta^2\dot{\delta}^2 - 2\beta\dot{\beta}\delta\dot{\delta} + \delta^2\dot{\beta}^2 + \dot{\gamma}^2}{g^2}} = \sqrt{\frac{\beta^2\dot{\delta}^2 - 2\beta\dot{\beta}\delta\dot{\delta} + \delta^2\dot{\beta}^2 + \dot{\gamma}^2 \times \overline{\beta^2 + \gamma^2 + \delta^2}}{\beta^2 + \delta^2}} =$ $\sqrt{\frac{\beta^2\dot{\delta}^2 - 2\beta\dot{\beta}\delta\dot{\delta} + \delta^2\dot{\beta}^2 + \beta^2\dot{\gamma}^2 + \delta^2\dot{\gamma}^2 + \beta^2\dot{\beta}^2 + 2\beta\dot{\beta}\delta\dot{\delta} + \delta^2\dot{\delta}^2}{\beta^2 + \delta^2}} = \sqrt{\dot{\delta}^2 + \dot{\beta}^2 + \dot{\gamma}^2}$. Again, the accelerating force in $BA = \frac{\dot{z}}{\dot{t}}$ reſolved into the direction of the great circle BO at B is $\frac{\dot{z}}{\dot{t}} \times$ cof. $OBR = \frac{\dot{z}}{\dot{t}} \times \frac{\beta}{g}$, and that $\frac{\dot{x}}{\dot{t}}$ along BC reſolved into the ſame direction is $\frac{\dot{x}}{\dot{t}} \times \frac{\delta}{g}$, and the difference of theſe, or the accelerating force in the direction BO in the ſenſe from O towards $B = \frac{\beta\dot{z} - \delta\dot{x}}{g\dot{t}} = \frac{\beta \times \overline{e\dot{\delta} + \dot{\delta e}} - \delta \times \overline{e\dot{\beta} + \beta\dot{e}}}{g\dot{t}} =$ $e \times \frac{\beta\dot{\delta} - \delta\dot{\beta}}{g\dot{t}}$; in like manner that along CO in the ſenſe from O towards $C = \frac{\gamma\dot{x} - \beta\dot{y}}{d\dot{t}} = e \times \frac{\gamma\dot{\beta} - \beta\dot{\gamma}}{d\dot{t}}$, and that along OA from O towards $A = \frac{\delta\dot{y} - \gamma\dot{z}}{b\dot{t}} = e \times \frac{\delta\dot{\gamma} - \gamma\dot{\delta}}{b\dot{t}}$; and as f. ROA (fig. 3.) = f. COA $= \frac{\gamma}{bd} : 1 ::$ this laſt mentioned force : $de \times \frac{\dot{\delta}\gamma - \gamma\dot{\delta}}{\gamma\dot{t}} =$ one equivalent thereto, but acting perpendicular to CO, and urging from O, that is, drawing the great circle DOE perpendicular to BO; then, as 1 : f. DOC = f. ROE = $\frac{\delta\gamma}{gd} ::$ this laſt force : the

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ſame reduced into the direction OE $= e\delta \times \frac{\delta\dot{\gamma} - \gamma\dot{\delta}}{g\dot{t}}$ acting perpendicular to the great circle BO, and in the ſenſe from O towards E: the ſame force reduced into the direction of the great circle BO at O is $= e\beta \times \frac{\delta\dot{\gamma} - \gamma\dot{\delta}}{g\gamma\dot{t}}$ in the ſenſe from O towards Q: in like manner is found a force equivalent to that in CO, but acting perpendicular to AO $= ebd \times \frac{\gamma\dot{\beta} - \beta\dot{\gamma}}{d\gamma\dot{t}}$, which reduced into the direction OD is $= e\beta \times \frac{\gamma\dot{\beta} - \beta\dot{\gamma}}{g\dot{t}}$ in the ſenſe from O towards D; but this ſame force perpendicular to AO, when reduced into the direction BO, is $= e\delta \times \frac{\gamma\dot{\beta} - \beta\dot{\gamma}}{g\gamma\dot{t}}$ in the ſenſe from O towards Q, which being added to the other above found force in BO gives $\frac{e\delta \times \gamma\dot{\beta} - \beta\dot{\gamma} + e\beta \times \delta\dot{\gamma} - \gamma\dot{\delta}}{g\gamma\dot{t}} = -e \times \frac{\beta\dot{\delta} - \delta\dot{\beta}}{g\dot{t}} =$ the accelerating force ariſing from thoſe which act at O along the great circles OA, OC, which force acts in the ſenſe from O towards Q, and therefore in a contrary ſenſe, that is, from O towards B it muſt be $= e \times \frac{\beta\dot{\delta} - \delta\dot{\beta}}{g\dot{t}}$ as before found, the operation thus proving itſelf. In like manner, from the two forces now found, which act perpendicular to OB at O, there muſt ariſe one acting along OD in the ſenſe from O towards D, which will therefore be $=$ $\frac{e\beta \times \gamma\dot{\beta} - \beta\dot{\gamma} - e\delta \times \delta\dot{\gamma} - \gamma\dot{\delta}}{g\dot{t}} = \frac{e}{g\dot{t}} \times \gamma\beta\dot{\beta} - \beta\beta\dot{\gamma} - \delta\delta\dot{\gamma} + \gamma\delta\dot{\delta} = \frac{e}{g\dot{t}} \times -\gamma\dot{\gamma}^2 - \beta^2\dot{\gamma} - \delta^2\dot{\gamma} = -\frac{e\dot{\gamma}}{g\dot{t}}$. This laſt force may be otherwiſe found thus, the acceleration $= \dot{y}$ round B at Q, and as $1 : g :: \dot{y} : g\dot{y} =$ the acceleration round B at O owing to $\dot{y}$, in like manner, the acceleration round C at O owing to $\dot{z}$ is $= d\dot{z}$, which reſolved

into the direction perpendicular to BO at O is $= d\dot{z} \times$ f. ROE $= \frac{\gamma\delta\dot{z}}{g}$, alſo the acceleration $b\dot{x}$ at O perpendicular to AO reduced into the direction perpendicular to BO $= b\dot{x} \times$ f. DOS $= \frac{\gamma\beta\dot{x}}{g}$, hence the whole acceleration along DE at O, which manifeſtly ariſes from theſe three, is $= \frac{\gamma\beta\dot{x}}{g} + \frac{\gamma\delta\dot{z}}{g} - g\dot{y}$, and the accelerative force $= \frac{\gamma\beta\dot{x}+\gamma\delta\dot{z}-g^2\dot{y}}{g\dot{t}}$ which, properly reduced, becomes $-\frac{e\dot{\gamma}}{g\dot{t}}$ as before. And the force which is compounded of the two forces $e \times \frac{\beta\dot{\delta}-\delta\dot{\beta}}{g\dot{t}}$ and $-\frac{e\dot{\gamma}}{g\dot{t}}$ is $= \frac{e}{g\dot{t}}\sqrt{\overline{\beta\dot{\delta}-\delta\dot{\beta}}^2+\dot{\gamma}^2} = \frac{e}{\dot{t}}\sqrt{\dot{\beta}^2+\dot{\gamma}^2+\dot{\delta}^2}$ acting perpendicular to the track of O upon the moving ſpherical ſurface; and $\frac{\dot{e}}{\dot{t}} = \frac{\beta\dot{x}+\gamma\dot{y}+\delta\dot{z}}{\dot{t}}$ is the accelerating force acting along the midcircle, or that which is 90° diſtant from O, to alter the velocity about the natural or momentary axis whoſe pole is O. Hence, anſwerable to the three accelerating forces $\frac{\dot{x}}{\dot{t}}$, $\frac{\dot{y}}{\dot{t}}$, and $\frac{\dot{z}}{\dot{t}}$, round the axes whoſe poles or ends A, B, and C, are always the ſame invariable points upon the moving ſpherical ſurface, there ariſe three other accelerating forces, namely, $e \times \frac{\beta\dot{\delta}-\delta\dot{\beta}}{g\dot{t}}$, $-\frac{e\dot{\gamma}}{g\dot{t}}$, and $\frac{\beta\dot{x}+\gamma\dot{y}+\delta\dot{z}}{\dot{t}}$; the two former acting at the pole of the momentary axis, and the latter is that whereby the velocity about the momentary axis is altered.

SCHOLIUM I.

From the preceding inveſtigation of the forces $e \times \frac{\beta\dot{\delta}-\delta\dot{\beta}}{g\dot{t}}$ and $-\frac{e\dot{\gamma}}{g\dot{t}}$, it follows, that they are not at all affected in expreſſion by the

the variation of e, but are denoted by the ſame quantities, whether e be conſtant or variable; which concluſion, and alſo the values of the forces themſelves, is perfectly agreeable to what is brought out by Mr. LANDEN, by a method ſo very different, in the Philoſophical Tranſactions for 1777.

But it is here carefully to be noted, that theſe are not motive forces, but accelerative ones; for no notice whatever is yet taken of the internal ſtructure of the revolving globe; but the expreſſions hold true, be that ſtructure what it will: if it be ſuch that one and the ſame quantity, drawn into each accelerating force, will give the correſpondent motive one, then are the motive forces proportional to the accelerative ones, but otherwiſe not. It may here alſo be obſerved, that it is quite conformable to nature, that theſe accelerating forces ſhould be expreſſed by the ſame quantities whether e be conſtant or variable; for theſe forces, acting at the pole of the natural axis, cannot poſſibly have any effect upon the velocity round it. But it is not hence by any means to be concluded, that the velocity about the axis is therefore conſtant; becauſe theſe are not, in general, the only accelerating forces that act upon the body, but there is alſo a third accelerating force whoſe value is $\frac{\dot{e}}{i}$ ariſing from the different variability of x, y, and z, and which cannot vaniſh except $\beta\dot{x}+\gamma\dot{y}+\delta\dot{z}=0$, it therefore can only vaniſh in particular caſes.

If the equation $\dot{e}=\beta\dot{x}+\gamma\dot{y}+\delta\dot{z}$ be ſquared, there will thence ariſe after due ordering $\dot{e}^2=\dot{x}^2+\dot{y}^2+\dot{z}^2-e^2\times(\overline{\gamma\dot{\beta}-\beta\dot{\gamma}}^2+\overline{\beta\dot{\delta}-\delta\dot{\beta}}^2+\overline{\delta\dot{\gamma}-\gamma\dot{\delta}}^2)$, where the member which is drawn into e^2 keeps its form whether e be conſtant or variable, but by no means will $\dot{x}^2+\dot{y}^2+\dot{z}^2$, after due ſubſtitution, do ſo too.

too. If $e=0$, then $\dot{e}^2=\dot{x}^2+\dot{y}^2+\dot{z}^2$, the motion being then round what M. EULER and M. D'ALEMBERT call the *initial axis*, or that about which the body at reſt would be firſt urged to move by any external forces acting upon it; and which they have determined with ſo much labour; though here it follows, as a neceſſary conſequence, that the force with which the body is turned round this initial axis is $=\sqrt{\frac{\dot{x}^2}{\dot{t}^2}+\frac{\dot{y}^2}{\dot{t}^2}+\frac{\dot{z}^2}{\dot{t}^2}}$, or a force = the ſum of the forces round the axes whoſe poles are A, B, and C.

Moreover, by the general laws of all motion, $\frac{\beta\dot{\delta}-\delta\dot{\beta}}{g\dot{t}}$, $-\frac{\dot{\gamma}}{g\dot{t}}$, and $\sqrt{\frac{\dot{\beta}^2+\dot{\gamma}^2+\dot{\delta}^2}{\dot{t}^2}}$ are the velocities with which the pole of the momentary axis ſhifts its place in directions perpendicular to BO, along BO and along its own track on the ſurface reſpectively. And it is by taking the fluxions of theſe, and dividing each fluxion by that of the time, that the accelerating forces are had, which are due to ſuch alteration of place of the momentary pole; and theſe muſt by no means be confounded with the forces before found $-\frac{e\dot{\gamma}}{g\dot{t}}$ and $\frac{e}{g\dot{t}}\times\overline{\beta\dot{\delta}-\delta\dot{\beta}}$ in thoſe directions, theſe laſt pertaining to the tendency of the ſurface itſelf to motion at O, and the others to the ſhifting of the pole of the axis upon the ſurface, which are different motions, as will more clearly appear from what follows.

The preceding general properties of motion obtain in all bodies revolving round a center at reſt, be their motion ever ſo irregular; the three great circles bounding an octant of the ſpherical ſurface revolving with the body are alſo taken *ad libitum*, being any ſuch circles whatever upon the ſurface; and hence the following very important conſequence is drawn, *viz.*

If any body be in motion, or put in motion, by inſtantaneous impulſe or otherwiſe, about its center of gravity at reſt in abſolute ſpace, if, by any means, the accelerating forces acting along the three great circles bounding any octant of a ſpherical ſurface that has the ſame center of gravity and revolves with the body, can be found, thoſe acting at every other point of ſuch ſurface will neceſſarily follow as natural conſequences of theſe three, and thus all the motions of ſuch body will be abſolutely determined.

SCHOLIUM II.

As the above concluſions are exceedingly general, in order to form a diſtinct idea how ſuch ſurface moves, it may be proper here to illuſtrate it by a particular example. Let then the velocity x be ſuppoſed conſtant, and alſo the angular velocity e; then, from what is ſhewn above, ſince $x\dot{x}=0$, $e^2=\dot{x}^2+y^2+z^2$ $=e^2\times\overline{\beta^2+\gamma^2+\delta^2}$, $e\dot{e}=y\dot{y}+z\dot{z}=0=e^2\times\overline{\gamma\dot{\gamma}+\delta\dot{\delta}}$, $\gamma\dot{\gamma}+\delta\dot{\delta}=0$, $\dot{\beta}=0$, β a conſtant quantity, therefore b is conſtant, and the track of the point O upon the ſurface is a leſſer circle of the ſphere at the conſtant diſtance AO from the invariable point A of the ſurface, the radius of ſuch leſſer circle being $=b=$ſ. AO (fig. 4.), alſo $y^2+z^2=$the conſtant quantity $e^2-x^2=e^2-e^2\beta^2=$ $e^2b^2=e^2\times\overline{\gamma^2+\delta^2}$, $z\dot{z}=-y\dot{y}$, $\dot{\delta}\dot{\delta}=-\gamma\dot{\gamma}=g\dot{g}$, and the velocity $\sqrt{\frac{\dot{\beta}^2+\dot{\gamma}^2+\dot{\delta}^2}{i^2}}$ with which the pole O ſhifts its place $=$ $\sqrt{\frac{\dot{\gamma}^2+\dot{\delta}^2}{i^2}}=\sqrt{\frac{\delta^2\dot{\delta}^2+\gamma^2\dot{\delta}^2}{\gamma^2i^2}}=\frac{b\dot{\delta}}{\gamma i}=\frac{b\dot{\delta}}{i\sqrt{b^2-\delta^2}}=\frac{\dot{\mathrm{EO}}}{i}$. But ſtill an expreſſion for i is wanting; to the two preceding *data* it is therefore neceſſary to add a third, which may be that the velocity with which O ſhifts its place in the circle EOF is alſo conſtant. Which will

will come to the ſame as a caſe occurring hereafter, when $\frac{\dot{x}}{\dot{t}}=0$, $\frac{\dot{y}}{\dot{t}}=-\frac{xz}{A}$ and $\frac{\dot{z}}{\dot{t}}=\frac{xy}{A}$ where A is ſome conſtant quantity; for then $\dot{x}=0=e\dot{\beta}+\beta\dot{e}$ $e\dot{e}=x\dot{x}+y\dot{y}+z\dot{z}=y\dot{y}+z\dot{z}=e\gamma\dot{y}+e\delta\dot{z}$, $\dot{e}=\gamma\dot{y}+\delta\dot{z}=-\frac{\gamma xz\dot{t}}{A}+\frac{\delta xy\dot{t}}{A}=\frac{e^2\dot{t}}{A}\times\overline{-\gamma\beta\delta+\gamma\beta\delta}=0$, therefore e is conſtant, and $\dot{t}=\frac{A\dot{z}}{xy}=\frac{Ae\dot{\delta}}{e^2\beta\gamma}=\frac{A\dot{\delta}}{e\beta\gamma}$, and ſince $\dot{e}=0$, and $\dot{x}=e\dot{\beta}+\beta\dot{e}=0=e\dot{\beta}$; therefore $\dot{\beta}=0$, β conſtant, and $\gamma=\sqrt{b^2-\delta^2}$; therefore $\dot{t}=\frac{A}{eb\beta}\times\frac{b\dot{\delta}}{\sqrt{b^2-\delta^2}}$, and $t=\frac{A}{eb\beta}\times$ arc EO; conſequently, the velocity with which O ſhifts its place in the arch EO is $=\frac{eb\beta}{A}$, which is a conſtant quantity.

PROPOSITION V.

The ſame being given, as in the laſt propoſition, it is propoſed to illuſtrate the manner in which the ſurface moves with reſpect to a point at reſt in abſolute ſpace.

Let Z (fig. 4.) be a point touching the ſurface, but at reſt in abſolute ſpace whilſt the ſurface moves under it in any manner whatſoever. In any one poſition of the octant ABC through Z draw the great circles As, Bq, and Cr, which by the property of the ſphere muſt be perpendicular to BC, CA, and AB, reſpectively; then muſt the velocities of the ſpherical ſurface at s, q, and r, in directions perpendicular to each of the circles As, Bq, and Cr, be x, y, and z, the angular velocities therefore about Z, with which the ſurface paſſes under s, q, and r, muſt be $\frac{x}{\text{f. Z}s}$, $\frac{y}{\text{f. Z}q}$, and $\frac{z}{\text{f. Z}r}$; through Z and O draw the quadrant of a great circle ZY; then, as $\beta : x ::$ f. OY

: $e \times$ f. OY = the velocity of the moving ſpherical ſurface at Y, which is therefore the angular velocity of the ſurface at Y round an axis at reſt whoſe pole is Z, becauſe ZY = 90°; which four values obtain, let the point Z be taken at reſt in abſolute ſpace whereſoever it will. Alſo, $e \times$ f. OZ is the velocity with which the ſurface paſſes under Z in a direction perpendicular to the great circle OZ at Z, which muſt therefore be the real velocity of the ſurface itſelf there at that inſtant; therefore the fluxion of the track upon the ſurface which continually paſſes under Z is $= e \times$ f. OZ $\times t =$ $\sqrt{\text{f. Z}\dot{s}^2 + \text{f. Z}\dot{q}^2 + \text{f. Z}\dot{r}^2}$ From which equation, and the properties of O found in the preceding propoſitions, general expreſſions for the relation of Z and O may be obtained. But, ſeeing that there is ſuch a latitude in determining or fixing upon a proper point Z out of an infinity of points at reſt, and this handled in a general manner will run into a complex *calculus*; in order to fix upon a point Z under the moſt eligible conditions, it may be beſt to deduce them from the properties of any particular problem that comes under conſideration.

For example, taking that in the ſecond ſcholium to the laſt propoſition, where x and e are conſtant, and $y^2 + z^2 = e^2 - x^2$ is alſo conſtant and $= e^2\gamma^2 + e^2\delta^2 = e^2 - e^2\beta^2 = e^2b^2$, or $\gamma^2 + \delta^2 = b^2$ alſo conſtant; and the velocity with which O ſhifts its place along its proper track $= \frac{eb\beta}{A}$, conſtant alſo. Here, in order to fix upon a proper point Z, ſuppoſe the motion to begin when O (fig. 5.) is upon the great circle AB at E, and after ſome determinate time $= t$, ſuppoſe the octant ABC to have arrived in the poſition A′B′C′, and that in this time the point O has ſhifted its place from E to O, that is, ſuppoſing the octant ABC to be at reſt in abſolute ſpace, while A′B′C′ is in motion, on A′B′

taking $A'e = AE$, the point O will have ſhifted its place in the time t, in abſolute ſpace from E to O; and upon the moving ſpherical ſurface from e to O along a leſs circle whoſe radius is equal to the ſine of $AE = \text{f. } A'e = \text{f. } A'O = b$. Now, at the commencement of the motion, that is, at AB, the firſt velocity of the point A along CA is $e \times \text{f. } AE = eb =$ the then value of y, becauſe the pole of the natural axis of motion E being then upon AB, the value of $z = 0$, and the pole E ſhifting its place in the ſenſe EO in abſolute ſpace, and the invariable point A of the ſpherical ſurface moving in the ſenſe AA′, there muſt be ſome point Z between E and A at reſt with reſpect to both theſe motions, or round which both of them may be ſuppoſed performed; its property muſt then be ſuch, that as f. AZ : f. EZ :· velocity of $A = eb$: to the velocity with which the pole E begins to ſhift its place $= eb \times \frac{\text{f. EZ}}{\text{f. AZ}}$; but $\frac{eb\beta}{A}$ is the velocity with which it ſhifts its place upon the moving ſpherical ſurface at E about the radius $= \text{f. } AE = \text{f. } \overline{AZ + EZ}$. And when O is at E the velocity with which the ſpherical ſurface paſſes under Z will be $e \times \text{f. } EZ$. Again, when O is the place of the momentary pole, the velocity of the point $A' = \sqrt{y^2 + z^2} = e\sqrt{\gamma^2 + \delta^2} = eb$ as before, and the velocity with which O ſhifts it place round $A' = \frac{eb\beta}{A}$ as before; it therefore ſhifts its place round ſome point Z in abſolute ſpace ſuch that $eb \times \frac{\text{f. OZ}}{\text{f. AZ}}$ is ſtill the velocity with which it ſhifts it, which, becauſe $A'O = AE$, muſt be the ſame velocity and the ſame point Z as before. Conſequently, the point O ſhifts its place along a leſſer circle of the ſphere whoſe radius $= \text{f. } EZ = \text{f. } OZ$, and in the time of ſuch ſhifting from E to O, or from A to A′, the point of the

ſurface which at firſt was under Z will arrive at z in A′B′, where $A'z = AZ$, and E conſidered as the ſame invariable point of the ſurface will arrive at e, ſo that $A'e = AE$; therefore, ſince $EZ = OZ$ is conſtant, and Z at reſt both with with reſpect to the velocity eb of A′ round it, and the velocity $\frac{eb\beta}{A}$ with which O ſhifts its place, it muſt be as ſ. EZ = ſ. OZ : ſ. AZ :: $\frac{eb\beta}{A} : eb :: \frac{\beta}{A} : 1$, but b and β are the ſine and coſine of $AE = A'O = EZ + AZ$; therefore, as ſ. $EZ = b \times$ coſ. $AZ - \beta \times$ ſ. AZ : $\beta \times$ ſ. AZ :: $\frac{1}{A} : 1$, and as $b \times$ coſ. AZ : $\beta \times$ ſ. AZ :: $\frac{1}{A} + 1 : 1 :: \frac{b}{\beta} =$ tang. AE : tang. $AZ = \frac{b}{\beta} \times \frac{A}{A+1}$, and ſ. AZ : coſ. $AZ =$ ſ. $BZ :: bA : \overline{A+1} \times \beta ::$ tang. AE : $\frac{A+1}{A}$, ſ. $AZ = \frac{Ab}{\sqrt{A^2 + 2A\beta^2 + \beta^2}}$, and coſ. $AZ = \frac{\overline{A+1} \times \beta}{\sqrt{A^2 + 2A\beta^2 + \beta^2}}$; and thus a diſtinct idea of this motion of the ſpherical ſurface is obtained, it being now clear, that the point A′ moves round Z at reſt with the velocity eb, and as ſ. ZA : 1 :: $eb : \frac{e\sqrt{A^2 + 2A\beta^2 + \beta^2}}{A} =$ the angular velocity with which A′ moves round the axis whoſe pole is Z, which is therefore conſtant; and at the ſame time the ſurface itſelf moves in the direction of the great circle B′C′, that is about the axis whoſe pole is A′ with the conſtant velocity $x = e\beta$, which two motions may be conſidered as ſeparate, and the reſt as conſequences of them; that is, the point Z is at reſt, and the point A′ moves uniformly round it, whilſt the ſurface upon which A′ is an invariable point moves round the axis whoſe pole is A′ with an uniform angular velocity, theſe two angular velocities being in the ratio of

$\frac{\sqrt{A^2+2A\beta^2+\beta^2}}{A} : \beta$, or of $\sqrt{A^2b^2+\overline{A+1}^2\times\beta^2} : A\beta$; therefore, the times being inverſely as the velocities, as $A\beta : \sqrt{A^2+2A\beta^2+\beta^2}$ :: the time of one revolution of A′ round Z : the time of one revolution of the ſurface round A′, that is, round the axis whoſe pole is A′, which time is given becauſe $x=e\beta$, and conſequently the time of one revolution of A′ round Z is given. Again, $e\times$ f. $OZ=\frac{eb\beta}{\sqrt{A^2+2A\beta^2+\beta^2}}$ = the velocity with which the ſurface paſſes under Z (at reſt). The angular velocity round the axis whoſe pole is $O=e$, and the velocity round O in a circle whoſe radius is $b=be$, O ſhifts its place in a circle of the ſame radius b with a velocity $=\frac{eb\beta}{A}$; the time therefore in which O ſhifts through the leſſer circle eO is to that of one revolution round O (which time may be ſuppoſed given) $=T$ as $eb :: \frac{eb\beta}{A}$, or as $1 : \frac{\beta}{A}$, that is, as $\frac{\beta}{A} : 1 :: T : \frac{AT}{\beta}$ = the time in which O makes one revolution upon the ſurface. And as $e\beta : T :: e : \frac{T}{\beta}$ = the time in which the ſurface makes one revolution round A′, or the axis whoſe pole is A′; and from the analogy above, the time of one revolution of A′ round $Z=\frac{AT}{\sqrt{A^2+2A\beta^2+\beta^2}}$. Alſo, as 1 : f. $AZ ::$ $e\beta : \frac{Aeb\beta}{\sqrt{A^2+2A\beta^2+\beta^2}}$ = the velocity with which the ſurface would paſs under Z, owing to the motion only round the axis whoſe pole is A′, and in a ſenſe from B′ towards C′; whereas, owing to the compound motion it really moves under Z in a contrary ſenſe with the velocity $\frac{eb\beta}{\sqrt{A^2+2A\beta^2+\beta^2}}$; this is, however, only a neceſſary conſequence of the centres of the circles whoſe radii

are f. A′Z and f. EZ ſhifting their places in abſolute ſpace, which therefore can in no wiſe affect the velocities round thoſe centres, which velocities muſt ſtill be the ſame relatively to the centres as if the centres were at reſt. Hence, then, the nature of this ſpherical motion is ſuch, that the axis whoſe pole is Z being abſolutely at reſt, the pole O ſo ſhifts its place in a circle whoſe radius = f. ZO alſo at reſt, as to do ſo with a conſtant velocity $= eb \times \frac{\text{f. EZ}}{\text{f. AZ}} = \frac{eb\beta}{A}$ = the velocity with which it ſhifts its place in the circle eO on the moving ſurface, the track therefore on the moving ſurface oſculates or rolls upon that on the immoveable one. Therefore, ſince $\frac{AT}{\beta}$ = the time of one revolution of O upon the moving ſurface, and the time of one revolution of A′, and conſequently O round Z = $\frac{AT}{\sqrt{A^2+2A\beta^2+\beta^2}}$; in the time of one revolution of O on the moving ſurface, it will have ſhifted its place round Z in the circle whoſe radius = f. OZ, through an arc = the whole periphery $\times \frac{\sqrt{A^2+2A\beta^2+\beta^2}}{\beta}$, that is, it will have made $\sqrt{\frac{A^2}{\beta^2}+2A+1}$ revolutions round Z: for, as the two circles eO and EO oſculate, it will take $\frac{\text{f. OA}}{\text{f. OZ}} = \sqrt{\frac{A^2}{\beta^2}+2A+1}$ times the periphery of EO to go round eO, that is, the point A′, and conſequently O will have moved this number of times round Z at reſt, whilſt O ſhifts its place once round the ſpherical ſurface in motion. Hence then the nature of the motion round the momentary axis whoſe pole is O, and the fixed one whoſe pole is Z, will be apparent from the following ſimple contrivance. A circle EO to radius = f. ZO = f. ZE being drawn upon a ſpherical ſurface at reſt, an octant of which is ABC, let a paper, or

other

other looſe ſurface, be fitted to this octant, and having on the centre A and radius AE deſcribed a circular arc on the looſe ſurface, let the part thereof EOFCBE be cut away, and completing the circle EF of the remaining part, let the circumference of this circle be moved uniformly along the circumference of the leſs fixed circle EO with the celerity $\frac{eb\beta}{A}$ beginning at the point E in each, ſo that the moving circle may roll along the fixed one, that is, ſo that the arc O*e* of the moving circle which has been in contact with the fixed one may be always equal to the arc EO of the fixed one with which it has been in contact; then, ſince OZ and ZA′ are conſtant, and OZ perpendicular to both circles, the point A′ muſt deſcribe upon the fixed ſurface, the ſame *locus* as in the caſe of the motion above ſpecified. The *locus* alſo of the momentary pole O will be the ſame, and the angular velocity of A′ about the momentary axis the ſame as that of the moving ſurface about it: for the celerity of O about the axis whoſe pole is $Z = \frac{eb\beta}{A}$ being equal to the celerity about A′ in motion, and the *locus* of A′ being a circle whoſe radius = ſ. ZA, we have, as ſ. ZO : ſ. ZA′ :: $\frac{eb\beta}{A}$: *eb* = the velocity of A′, and as ſ. OA′ = *b* : *eb* :: radius = 1 : *e* = the velocity about the momentary axis, as it ought.

From this complete ſolution of the particular caſe may be collected in general, that if the octant ABC be taken ſuch upon the moving ſpherical ſurface, that the track of O thereupon may croſs the two great circles AB and AC at right angles, a point which is at reſt with reſpect to both motions, or round which they are performed like a ſingle motion, may at the inſtant of the momentary pole's croſſing each of thoſe

great

great circles be found, in the ſame manner as in the particular caſe here ſpecified. And it will alſo be found for any poſition of O, by means of the expreſſions for the velocities found in Scholium I. Prop. IV.; but of this more hereafter.

PROPOSITION VI.

If a parallelopipedon (or other * ſolid) revolving uniformly with an angular velocity $= z$ about one of its permanent axes of rotation, receive an inſtantaneous impulſe in a direction parallel to that axis, the centre of gravity of the body being ſuppoſed to be kept at reſt by an equal and contrary impulſe given to it, and no other force acting upon the body, it is propoſed to determine the alteration in the motion thereof, in conſequence of ſuch inſtantaneous impulſe.

The impulſe being, by hypotheſis, given in a direction perpendicular to that of the then only motion of every particle of the body, cannot inſtantly alter its angular velocity about the permanent axis; but its immediate effect muſt be to cauſe the body to revolve about a freſh axis, whilſt the angular velocity, and conſequently the *momentum* of rotation about the firſt or permanent axis, remain unaltered by ſuch inſtantaneous impulſe; for though it gives a different direction and velocity to the particles, by cauſing them to revolve about another axis, yet muſt their relative velocity about the firſt remain unaltered by the nature of relative motion, becauſe the ſecond or additional motion is given in a direction perpendicular to the firſt. Any alteration therefore which may be made in the velocity about the firſt axis, by reaſon of the oblique motion of the particles about it, owing to the then revolution about a freſh axis, muſt be a work of time. And to determine ſuch alteration,

* See the *note* (C) at the end of the Paper.

let

let M = the maſs or ſolidity, and $2d$, $2c$, and $2b$, be the three dimenſions or length, breadth, and thickneſs of ſuch parallelopipedon; then it is known that the momentum of *inertia* round the axis on which the dimenſion $2d$ is taken will be $= \frac{1}{3} M \times \overline{c^2 + b^2}$, this being no more than the product of a particle of the body into the ſquare of its diſtance from ſuch axis, when integrated through the whole body, as is now too well known to need the repetition here. Let I (fig. 6.) be the centre of gravity or of *inertia* (they being both one) of ſuch parallelopipedon, IB the permanent axis on which the dimenſion $2c$ is taken, CI that on which $2b$ is taken, and a perpendicular to the plane BIC (of the paper) at I that on which $2d$ is taken; then on the centre I deſcribing the quadrant BSC, whoſe radius BI or CI may be ſuppoſed unity; if the body once revolve about this laſt named axis with an angular velocity $= z$ meaſured along the great circle BSC, and no external force or impulſe act upon it, it is agreed and well known, that the centrifugal motive force round ſuch axis will be $= Mz^2 \times \frac{c^2 + b^2}{3}$ and always being equal in contrary directions round the axis can have no power to alter the place thereof; but ſuch motion and motive force continuing always the ſame, the axis muſt be at reſt, and the velocity round it uniform for ever. But, if the body whilſt ſo revolving receive (as by hypotheſis) an impulſe in a direction parallel to this axis, that is, perpendicular to the plane of the circle BCI, and an equal and contrary one to keep the centre I ſtill at reſt, the ſaid impulſe being perpendicular to the motion cannot inſtantly alter the angular velocity z, but will give the axis itſelf a motion in a plane perpendicular to BCI, and conſequently about ſome axis SI in the plane BCI, round which axis SI the centrifugal motive

forces

forces of the particles being no longer in *equilibrio*, becauſe it is not a permanent axis (except in particular caſes) this oblique motion of the particles will in time alter the velocity z. To determine then the value of the motive force cauſing ſuch alteration of z, let ML $= 2d$ be a line parallel to the ſide of the parallelogram which is a ſection of the ſolid perpendicular to the axis CI, q the middle point of ML, p any other point therein, pm and qn two perpendiculars to the plane which is perpendicular to BCI and paſſes through SI; and from B let fall BN perpendicular to the axis SI: then, the point n muſt neceſſarily fall upon SI, becauſe the plane BSCI produced biſects the ſolid, join pn which is the perpendicular diſtance of p from the axis SI; let $v =$ the velocity of the body at B perpendicular to BI and to the plane BCI (which is the ſame invariable one in the body, and that wherein the permanent axes BI and CI are ſituated); then, as BN : v :: 1 : the angular velocity of the body about the axis SI $= \frac{v}{BN}$, and by the nature of all motion, as BN : v :: pn : $\frac{pn}{BN} \times v =$ the velocity of the point p round n, or of a particle of the body at p in the circle whoſe radius is pn, conſequently the centrifugal accelerating force, which is always equal to the ſquare of the velocity divided by the radius of motion, is there $= \frac{pn}{BN^2} \times v^2$ acting in the direction pn upon the axis SI, which may be reſolved into two others, the one parallel to the plane SmI, which can have no effect in a direction perpendicular to that plane, and the other $= \frac{v^2}{BN^2} \times pm = \frac{v^2}{BN^2} \times qn$ perpendicular to that plane, which drawn into a particle p of the body at p gives $p \times qn \times \frac{v^2}{BN^2} =$ the motive

force of that particle to move the plane S*m*I in a direction parallel to BN, or about the permanent axis which is perpendicular to the plane SBI, and which value is the ſame in whatever point of ML the particle p is ſituated.

Let GgR (fig. 7.) be a ſection of the ſolid by the plane IBSC; then, ſince the motive force of a particle p of the body ſituated any where in a line perpendicular to this plane at q is $p \times qn \times \frac{v^2}{BN^2}$, the motive force ariſing from the dimenſion ML = $2d$ of the body will be $= 2dv^2 \times \frac{qn}{BN^2}$, and as SI = 1 : I$n$ :: $2dv^2 \times \frac{qn}{BN^2}$: $2dv^2 \times \frac{qn \times In}{BN^2}$ = the equivalent motive force acting at the conſtant diſtance SI = unity; which muſt ſtill be integrated with the other two dimenſions of the body, becauſe every particle $p = M\dot{p} \times K\dot{R} \times q\dot{g}$. In order to which, let now $f = \frac{v^2}{BN^2}$, s and t = the ſine and coſine of QIK = NBI = SC to radius unity, IR = b, GK = c, KR = x, and $qg = y$; then will KI = $x - b$, Kq = $y - c$, and as t : KI :: 1 : qI = $\frac{x-b}{t}$ :: s : QK = $\frac{s}{t} \times \overline{x-b}$; hence, Q$q$ = Kq − QK = $y - c - \frac{s}{t} \times \overline{x-b}$, and 1 : Q$q$ :: s : Qn = $s \times \overline{y-c} - \frac{s^2}{t} \times \overline{x-b}$ :: t : qn = $t \times \overline{y-c} - s \times \overline{x-b}$, and I$n$ = QI + Qn = $s \times \overline{y-c} + t \times \overline{x-b}$; hence $qn \times In = st \times \overline{y^2 - 2yc + c^2} + t^2 \times \overline{y-c} \times \overline{x-b} - s^2 \times \overline{y-c} \times \overline{x-b} - st \times \overline{x-b}^2$, which multiplied into $2afy$ and the fluent making y only variable ſo as to comprehend the whole body, when $y = 2c = g$G, is $= 2dfst \times \frac{2c^3}{3} - 2c \times \overline{x^2 - 2bx + b^2}$, and this multiplied into $\dot{x}$, and the fluent taken in like manner, will, when $x = 2b$, be $= \frac{8}{3} \times dfst \times$

c^3

$\overline{c^3b-b^3c} = Mfst \times \frac{c^2-b^2}{3} = \frac{Mv^2st}{3t^2} \times \overline{c^2-b^2} = \frac{sv^2}{t} \times \frac{M}{3} \times \overline{c^2-b^2}$; but as f. BS$=t$: v :: f. CS$=s$: $\frac{sv}{t}$ = the velocity of the body at C perpendicular to CI and to the plane BCI; let $\frac{sv}{t} = x$ now, and $v=y$, and the preceding fluent becomes $\frac{Mxy}{3} \times \overline{c^2-b^2}$ = the motive force acting at S along the circle BSC to alter the velocity z along that circle; and if this be divided by the *inertia* $\frac{M}{3} \times \overline{c^2+b^2}$ along BC, it gives $xy \times \frac{c^2-b^2}{c^2+b^2} = \frac{\dot{z}}{\dot{t}}$ (where $\dot{t}$ = that of the time) = the accelerating force acting along the circle BC. Now (this being referred to fig. 3.), for the ſame reaſon, as the two velocities x and y along BC and CA turn the body about an axis whoſe pole is R in AB, and thus cauſe the perturbating motive force $\frac{Mxy}{3} \times \overline{c^2-b^2}$ above computed, muſt the two velocities x and z along BC and BA turn the body about an axis in CA whoſe pole is Q, and proceeding in the very ſame manner as before, the perturbating motive force thence ariſing will be found $= \frac{Mxz}{3} \times \overline{b^2-d^2}$, to alter the motion along AC, and the accelerative one $= \frac{b^2-d^2}{b^2+d^2} \times xz = \frac{\dot{y}}{\dot{t}}$ to alter the velocity y about the permanent axis whoſe pole is B. Alſo, the motive force $\frac{Myz}{3} \times \overline{d^2-c^2}$, and the accelerative one $= \frac{d^2-c^2}{d^2+c^2} \times yz = \frac{\dot{x}}{\dot{t}}$ to alter the velocity x along BC.

SCHOLIUM I.

Having thus obtained the values of the accelerating forces $\frac{\dot{x}}{\dot{t}}$, $\frac{\dot{y}}{\dot{t}}$, and $\frac{\dot{z}}{\dot{t}}$ (ſee Scholium I. prop. IV.), the matter is now

brought to an iſſue, and the motions and times may from hence be computed. But it will be proper firſt to ſhew wherein, and why, theſe concluſions differ from thoſe brought out by Mr. LANDEN.

The three perturbating motive forces acting along the peripheries of the three great circles CB, CA, and AB, in fig. 3. Prop. IV. are above found to be $\frac{M}{3} \times \overline{d^2 - c^2} \times yz$, $\frac{M}{3} \times \overline{b^2 - d^2} \times xz$, and $\frac{M}{3} \times \overline{c^2 - b^2} \times xy$ reſpectively, or their equals $\frac{M}{3} \times \overline{d^2 - c^2} \times e^2\gamma\delta$, $\frac{M}{3} \times \overline{b^2 - d^2} \times e^2\beta\delta$, and $\frac{M}{3} \times \overline{c^2 - b^2} \times e^2\beta\gamma$. And if we ſuppoſe the *accelerations* $\dot{x}$, $\dot{y}$, and $\dot{z}$, to be reſpectively proportional to the motive forces, the ſum $\dot{x} + \dot{y} + \dot{z}$ muſt be proportional to the ſum of the three motive forces, and $x\dot{x} + y\dot{y} + z\dot{z}$ or its equal $e\beta\dot{x} + e\gamma\dot{y} + e\delta\dot{z}$ muſt be proportional to $\frac{M}{3} \times \overline{d^2 - c^2} \times xe^2\gamma\delta + \frac{M}{3} \times \overline{b^2 - d^2} \times ye^2\beta\delta + \frac{M}{3} \times \overline{c^2 - b^2} \times ze^2\beta\gamma = \frac{M}{3} \times e^3\beta\gamma\delta \times \overline{d^2 - c^2 + b^2 - d^2 + c^2 - b^2}$ that is as nothing; conſequently, $e\dot{e} = x\dot{x} + y\dot{y} + z\dot{z} = 0$, in which caſe therefore e muſt be a conſtant quantity. Moreover, theſe quantities now mentioned as reſpectively proportional to one another, turning the equal ratios into equations $\frac{\overline{d^2 - c^2} \times yz}{\dot{x}} = \frac{\overline{b^2 - d^2} \times xz}{\dot{y}} = \frac{\overline{c^2 - b^2} \times xy}{\dot{z}} = \frac{\overline{d^2 - c^2} \times e\gamma\delta}{\dot{\beta}} = \frac{\overline{b^2 - d^2} \times e\beta\delta}{\dot{\gamma}} = \frac{\overline{c^2 - b^2} \times e\beta\gamma}{\dot{\delta}} = \frac{Be\gamma\delta}{\dot{\beta}} = \frac{De\beta\delta}{\dot{\gamma}} = \frac{Ce\beta\gamma}{\dot{\delta}}$; hence $D\beta\dot{\beta} = -B\gamma\dot{\gamma}$, and $D\delta\dot{\delta} = -C\gamma\dot{\gamma}$, and taking the fluents of theſe two laſt equations, putting n and m for the reſpective values of β and δ when $\gamma = 0$, we obtain $D\beta^2 = Dn^2 - B\gamma^2$, and $D\delta^2 = Dm^2 - C\gamma^2$, conſequently $\beta = \frac{\sqrt{Dn^2 - B\gamma^2}}{D^{\frac{1}{2}}}$ and $\delta = \frac{\sqrt{Dm^2 - C\gamma^2}}{D^{\frac{1}{2}}}$; which are the very

very equations brought out by Mr. LANDEN in ſo very different a manner.

Here then the matter may be ſafely reſted; for the accelerations are moſt certainly as the accelerative forces, and not as the motive ones. Concluſions, therefore, that are drawn from a contrary ſuppoſition cannot be true.

It may not, however, be improper to ſhew here how Mr. LANDEN's motive forces E and E″ ariſe from thoſe above brought out; thus, in fig. 3. Prop. IV. let s and t the ſine and coſine of AQ to radius 1, that is, let $s=\frac{\delta}{g}$ and $t=\frac{\beta}{g}$, then the motive force along BA reſolved into the direction BO becomes $\frac{M}{3}\times\overline{c^2-b^2}\times e^2\beta\gamma t$, and that along BC reſolved into the ſame direction BO becomes $\frac{M}{3}\times\overline{d^2-c^2}\times e^2\gamma\delta s$, the difference of theſe $=\frac{M}{3}\times e^2\gamma\times\overline{d^2-c^2\times\delta s-c^2-b^2\times\beta t}=\frac{Me^2g\gamma}{3}\times\overline{Ds^2-C}$ muſt be the motive force acting along the great circle BO in the ſenſe from B towards O, or from O towards Q; and this is the very motive force E determined by Mr. LANDEN, and acting in the ſame manner. The motive force which acts at O perpendicularly to the force E is moſt readily obtained from that acting along CA; for if a tangent be drawn to the great circle BOQ at O (fig. 3.) it will interſect a radius of the ſphere drawn through Q at a diſtance $\left(\frac{I}{g}\right)$ from I the centre of the ſphere = the ſecant of the arc OQ, and as $1:\frac{I}{g}=$ that ſecant :: the force $\frac{MDe^2\beta\delta}{3}$ acting at the diſtance Q from the centre : $\frac{MDe^2\beta\delta}{3g}=\frac{MDe^2gst}{3}$ the force acting in the plane of the great circle CIA at the diſtance $\frac{I}{g}$ from the centre I, and perpendicular to a tangent at

O

O to the great circle BOQ; which force being in a direction parallel to and in the ſame plane with the motive force acting at O perpendicular to the ſame tangent muſt be equal to it; that is, the motive force which acts perpendicular to E at O is = $\frac{Me^2g}{3} \times Dst$ = Mr. LANDEN's force E″. And this may alſo be deduced by finding by reſolution the motive forces along CO and AO, and reducing them into the direction of the great circle DOE at O, in the ſame manner as the accelerating forces are managed in Prop. IV. above. Now, theſe forces E and E″ not being the only perturbating ones that diſturb the motion of the body, but others ariſing from the *non-equilibrium* of the particles in motion round the axes which are perpendicular to the planes of the varying momentary great circles BOQ, DOE, they will neither divided by their reſpective *inertia* $\frac{M}{3} \times \sqrt{b^2+c^2+\sqrt{d^2-b^2}\,.\,s^2}$ and $\frac{M}{3} \times \sqrt{d^2+b^2+\gamma^2 \times c^2-b^2-s^2\,.\,a^2-b^2}$ give the accelerating forces along thoſe circles, nor are proportional to them; but, by the general properties of all motion as proved in Prop. IV. the accelerating forces in thoſe circles are $\frac{\delta}{g} \times \frac{\dot{x}}{\dot{t}} - \frac{\beta}{g} \times \frac{\dot{z}}{\dot{t}}$ ($\dot{t}$ = that of the time) $= \frac{s\dot{x}}{\dot{t}} - \frac{t\dot{z}}{\dot{t}} = \frac{d^2-c^2}{d^2+c^2} \times e^2 s\gamma\delta - \frac{c^2-b^2}{c^2+b^2} \times e^2 t\beta\gamma = \frac{d^2-c^2}{d^2+c^2} \times e^2 g\gamma s^2 - \frac{c^2-b^2}{c^2+b^2} \times e^2 g\gamma t^2 = \frac{e\delta\dot{\beta}}{g\dot{t}} - \frac{e\beta\dot{\delta}}{g\dot{t}}$ (S) and $\frac{\gamma\beta}{g} \times \frac{\dot{x}}{\dot{t}} + \frac{\gamma\delta}{g} \times \frac{\dot{z}}{\dot{t}} - \frac{g\dot{y}}{\dot{t}} = \frac{\gamma\beta}{g} \times \frac{d^2-c^2}{d^2+c^2} \times e^2\gamma\delta + \frac{\gamma\delta}{g} \times \frac{c^2-b^2}{c^2+b^2} \times e^2\beta\gamma + g \times \frac{d^2-b^2}{d^2+b^2} \times e^2\beta\delta = \frac{e^2\beta\delta}{g} \times \left(\frac{d^2-c^2}{d^2+c^2} + \frac{c^2-b^2}{c^2+b^2} - \frac{a^2-b^2}{a^2+b^2} \times \gamma^2 + \frac{d^2-b^2}{d^2+b^2}\right) = \frac{e^2\beta\delta}{g} \times \left(\frac{d^2-c^2}{d^2+c^2} \times \frac{c^2-b^2}{c^2+b^2} \times \frac{d^2-b^2}{a^2+b^2} \times \gamma^2 + \frac{d^2-b^2}{d^2+b^2}\right) = -\frac{e\dot{\gamma}}{g\dot{t}}$ (Q). And from theſe equations (S) and (Q) there reſults the analogy, as $\frac{d^2-c^2}{a^2+c^2} \times e^2 g\gamma s^2 - \frac{c^2-b^2}{c^2+b^2} \times e^2 g\gamma t^2 : \frac{e^2\beta\delta}{g} \times \left(\frac{a^2-c^2}{d^2+c^2} + \frac{c^2-b^2}{c^2-b^2} - \frac{d^2-b^2}{d^2+b^2} \times \gamma^2\right.$

+

$+\frac{d^2-b^2}{d^2+b^2}\Big) :: \frac{e\beta\dot{\beta}}{g} - \frac{e\beta\dot{\delta}}{g} : -\frac{e\dot{\gamma}}{g} :: \delta\dot{\beta} - \beta\dot{\delta} : -\dot{\gamma} :: \frac{\dot{\delta}\dot{\beta}-\beta\dot{\delta}}{g^2} = -\frac{\dot{s}}{t} : -\frac{\dot{\gamma}}{g^2}$;

hence, $\frac{a^2-c^2}{a^2+c^2} \times s^2 - \frac{c^2-b^2}{c^2+b^2} \times t^2 : \overline{\frac{a^2-c^2}{d^2+c^2} + \frac{c^2-b^2}{c^2+b^2}} \times \gamma^2 + \frac{d^2-b^2}{d^2+b^2} \times g^2 ::$

$-s\dot{s} : \frac{g\dot{g}}{g^2}$, or, putting $\frac{c^2-b^2}{c^2+b^2} = M^2 \times \overline{\frac{a^2-c}{a^2+c^2} + \frac{c^2-b^2}{c^2+b^2}}$ and $\frac{a^2-c^2}{d^2+c^2} +$

$\frac{c^2-b^2}{c^2+b^2} = N^2 \times \overline{\frac{d^2-c}{d^2+c^2} + \frac{c^2-c^2}{c^2+b^2} + \frac{b^2-d^2}{d^2+b^2}}$, $-s\dot{s} : \frac{g\dot{g}}{g^2} :: s^2 - M^2 : 1 - N^2g^2$,

or $\frac{-s\dot{s}}{s^2-M^2} = \frac{g\dot{g}}{g^2-N^2g^4}$; but when $g = 1$, $s = m$, and the fluents corrected accordingly, give the equation $\frac{1}{2}$ log. of $\frac{m^2-M^2}{s^2-M^2} =$ $\frac{1}{2}$ log. of $\frac{g^2-N^2g^4}{1-N^2} -$ log. of $\frac{1-N^2g^2}{1-N^2}$, consequently, $\sqrt{\frac{m^2-M^2}{s^2-M^2}} =$ $\sqrt{\frac{g^2-N^2g^2}{1-N^2g^2}}$, and $m^2 \times \overline{1-N^2g^2} - M^2 = s^2g^2 \times \overline{1-N^2} - M^2g^2$; but $sg = \delta$, therefore $\overline{m^2-\delta^2} \times \overline{1-N^2} = \overline{M^2-N^2m^2} \times \gamma^2$; or, expunging M and N, $m^2 - \delta^2 = \frac{\overline{d^2+b^2} \times \overline{c^2-b^2}}{\overline{c^2+b^2} \times \overline{d^2-b^2}} \times \gamma^2 - m^2\gamma^2 \times \frac{\overline{d^2-c} \times \overline{c^2-b^2}}{\overline{d^2+c^2} \times \overline{c^2+b^2}}$ is the equation of the curve whichis the locus of O upon the moving ſpherical ſurface; or, if $\frac{d^2+c^2}{d^2-c^2} = A$, $\frac{d^2+b^2}{d^2-b^2} = B$ and, $\frac{c^2+b^2}{c^2-b^2} = C$, $m^2 - \delta^2 = \frac{B\gamma^2}{C} - \frac{m^2\gamma^2}{AC}$. Which concluſion may be brought out with much more facility, by means of the three original equations above inveſtigated, which expreſs the values of the three accelerating forces $\frac{\dot{x}}{\dot{t}}$, $\frac{\dot{y}}{\dot{t}}$, and $\frac{\dot{z}}{\dot{t}}$, as will be ſhewn hereafter. But it is of importance to have proved here, that this different method when rightly treated comes to the ſame as the other.

SCHOLIUM

SCHOLIUM II.

The velocity of the body in directions of the peripheries of three great circles bounding an octant of the ſpherical ſurface which revolves with it, might have been referred to any other octant beſides that whoſe angles, as in the preceding ſolution, are in the poles of the three permanent axes; but then, beſides the perturbating force ariſing from the motion of the body about each of the three axes whoſe poles are in the nodes of the great circles bounding ſuch octant, there will, pertaining to each circle, be another perturbating force, ariſing from the *non-equilibrium* of the particles of the body in motion, in planes parallel to the plane of each circle, which being conſidered would greatly perplex the operation. And hence ariſes the neceſſity for referring the motion to permanent axes, becauſe about them this laſt-mentioned perturbating force vaniſhes by reaſon of the perfect *equilibrium* of the particles in motion round them; their property being ſuch, that if the body begin to move ſimply round one of them, it muſt uniformly continue ſo to do for ever. And if, as in the preceding propoſition, the body be compelled to move round ſome other axis, ſtill during the elementary time t, notwithſtanding that each of theſe axes or their poles has a proper motion of its own, yet the *relative* angular velocity, and conſequently the *inertia* and motive force round each axis, will be the ſame as if the body revolved with the ſingle angular velocity x, y, or z, round only one of them, and conſequently ſuch velocity can have no power to alter itſelf; but the *equilibrium* of the particles tends to preſerve it, for the particles by their motion round one of theſe axes cannot alter the angular velocity about it; but ſuch

alteration muſt be cauſed by the other motions of the body which are referred to the other two permanent axes as in the foregoing ſolution, and thus produce the forces $\frac{\dot{x}}{i}$, $\frac{\dot{y}}{i}$, and $\frac{\dot{z}}{i}$, acting at the nodes S, Q, and R, of the great circles BC, CA, and AB.

If the two great circles DOE, CQA, be continued, they will meet in a point of the midcircle 90° from O, and make an angle whoſe meaſure is the arc OQ, and if Mr. LANDEN's motive force E″ be reſolved into the direction of the great circle CA, it will become $E'' \times \text{cof. } OQ = E'' \times g = \frac{M}{3} \times \overline{a^2 - b^2} \times e^2 \beta\delta$, the very ſame as inveſtigated in the foregoing propoſition. But Mr. LANDEN's method, beſides the force E″ perpendicular to BO, will likewiſe give two other motive forces perpendicular to AO and CO at O, which reſolved into the directions of the great circles BC and AB will alſo give the above inveſtigated motive forces in thoſe circles, and thus the two methods prove each other.

I know then of no objection but what is already obviated; I ſhall therefore proceed to the ſolution of the following propoſition; firſt, independent of the conſideration of a momentary axis, the properties of which ſhall be inveſtigated afterwards. I could eaſily give the demonſtration that the properties above ſhewn to belong to the parallelopipedon, alſo pertain to any other body; but as this has been done before by Mr. LANDEN, I ſhall take it for granted here.

PROPOSITION VII.

If a body of any form revolve in any manner whatſoever with its centre of gravity at reſt in abſolute ſpace, and ſo as not to be diſturbed by the action of any external force; to determine in what manner it will continue its motion for ever.

Since any body whatever, whoſe permanent axes can be found, may be reduced to an equipollent parallelopipedon which ſhall move in the very ſame manner as the body; let this be ſuppoſed done, M being the maſs or ſolidity of the body, and Ma^2, Mb^2, and Mc^2, the reſpective *momenta* of *inertia* round the three permanent axes of the body whoſe poles in the ſpherical ſurface whoſe radius is unity revolving as the body revolves and concentric with it are A, B, and C, at the diſtance of a quadrant from each other (fig. 8.); let x = the velocity with which the body moves round the permanent axis whoſe pole is A, and meaſured along the great circle BC at the diſtance of a quadrant from that pole (A) and in the ſenſe from B towards C; in like manner, let y = the velocity round the axis whoſe pole is B, meaſured along CA, and in the ſenſe from C towards A, and z = that round the remaining permanent axis whoſe pole is C meaſured along AB, and in the ſenſe from A towards B. Alſo let t = the time from the commencement of the motion.

Then, the quantities which in the 6th Propoſition were repreſented by $\frac{M}{3} \times \sqrt{d^2+c^2}$, $\frac{M}{3} \times \sqrt{d^2+b^2}$, $\frac{M}{3} \times \sqrt{c^2+b^2}$, $\frac{M}{3} \times \sqrt{d^2-c^2}$, $\frac{M}{3} \times \sqrt{b^2-a^2}$, $\frac{M}{3} \times \sqrt{c^2-b^2}$, $\frac{d^2-c^2}{a^2+c^2}$, $\frac{b^2-d^2}{d^2+b^2}$, and $\frac{c^2-b^2}{c^2+b^2}$ reſpectively, muſt

muſt now become Ma^2, Mb^2, Mc^2, $M \times \overline{b^2 - c^2}$, $M \times \overline{c^2 - a^2}$, $M \times \overline{a^2 - b^2}$, $\frac{b^2 - c^2}{a^2}$, $\frac{c^2 - a^2}{b^2}$, and $\frac{a^2 - b^2}{c^2}$. And the three fundamental equations for the accelerative forces become $\frac{\overline{b^2 - c^2} \times yz}{a^2} = \frac{\dot{x}}{\dot{t}}$, $\frac{\overline{c^2 - a^2} \times zx}{b^2} = \frac{\dot{y}}{\dot{t}}$, and $\frac{\overline{a^2 - b^2} \times xy}{c^2} = \frac{\dot{z}}{\dot{t}}$, or $\dot{x} = \frac{\overline{b^2 - c^2} \times yz\dot{t}}{a^2}$, $\dot{y} = \frac{\overline{c^2 - a^2} \times zx\dot{t}}{b^2}$, $\dot{z} = \frac{\overline{a^2 - b^2} \times xy\dot{t}}{c^2}$; multiplying the firſt of theſe equations by a^2x, the ſecond by b^2y, and the third by c^2z, and adding all the three products or reſulting equations together gives $a^2x\dot{x} + b^2y\dot{y} + c^2z\dot{z} = 0$; alſo multiplying them reſpectively by a^4x, b^4y, and c^4z, and adding the three products produces $a^4x\dot{x} + b^4y\dot{y} + c^4z\dot{z} = 0$; and if $\mathfrak{A}$, $\mathfrak{B}$, and $\mathfrak{C}$, be the reſpective values of x, y, and z, at the commencement of the motion, taking the fluents $a^2x^2 + b^2y^2 + c^2z^2 = a^2\mathfrak{A}^2 + b^2\mathfrak{B}^2 + c^2\mathfrak{C}^2$, and $a^4x^2 + b^4y^2 + c^4z^2 = a^4\mathfrak{A}^2 + b^4\mathfrak{B}^2 + c\,\mathfrak{C}^2$, which therefore are conſtant quantities. But Ma^2x^2, Mb^2y^2, and Mc^2z^2, are the reſpective *vires vivæ* of the body round the three permanent axes, and conſequently their ſum, or the whole *vis viva* is always the ſame conſtant quantity. Alſo, ſince $\dot{t} = \frac{a^2\dot{x}}{\overline{b^2 - c^2} \times yz} = \frac{c^2\dot{z}}{\overline{a^2 - b^2} \times xy} = \frac{b^2\dot{y}}{\overline{c^2 - a^2} \cdot xz}$, therefore $\frac{a^2x\dot{x}}{b^2 - c^2} = \frac{b^2y\dot{y}}{c^2 - a^2} = \frac{c^2z\dot{z}}{a^2 - b^2}$, and the fluents $\frac{a^2}{b^2 - c^2} \times \overline{x^2 - \mathfrak{A}^2} = \frac{b^2}{c^2 - a^2} \times \overline{y^2 - \mathfrak{B}^2} = \frac{c^2}{a^2 - b^2} \times \overline{z^2 - \mathfrak{C}^2}$; hence then $y = \sqrt{\frac{a^2 \times \overline{c^2 - a^2}}{b^2 \times \overline{b^2 - c^2}} \times \overline{x^2 - \mathfrak{A}^2} + \mathfrak{B}^2}$, and $z = \sqrt{\frac{a^2 \times \overline{a^2 - b^2}}{c^2 \times \overline{b^2 - c^2}} \times \overline{x^2 - \mathfrak{A}^2} + \mathfrak{C}^2}$, which values ſubſtituted for y and z in the equation $\dot{t} = \frac{a^2\dot{x}}{\overline{b^2 - c^2} \times yz}$, give t in terms of x, $\dot{x}$ and conſtant quantities. But the fluent, though attainable by means of the arcs of the

conic ſections, is inſufficient for determining the motion of the body with reſpect to abſolute ſpace, becauſe at preſent nothing is found but the relations of *inertiæ* and velocities.

In order to determine a point which can be conſidered as at reſt in abſolute ſpace, and the nature of the body's motion with reſpect to it; let Z (fig. 8.) be ſuch a point, abſolutely at reſt itſelf, but ſo as to be always touched by the moving ſpherical ſurface which revolves with the body. Or, it is the ſame thing to conſider it as a given point upon a concave ſpherical ſurface at reſt, ſurrounding and every where touching that ſuppoſed above to revolve with the body. Through this point Z ſuppoſe quadrantal arcs A*l*, B*m*, and C*n*, to be drawn from the poles of the three permanent axes, and conſequently perpendicular to the three ſides of the octant ABC, ſuppoſing alſo Z to be at the inſtant over ſome point of this octant, and that *a* is greater than *b*, and *b* than *c*, when the velocity of the octant along its three ſides muſt neceſſarily be in the ſenſe from A towards B, from B towards C, and from C towards A; then (by ſpherics) as f. ZA : 1 :: f. Z*m* = cof. ZB : f. ZAC = cof. ZAB :; f. Z*n* = cof. ZC : f. ZAB = cof. ZAC; alſo, as f. BZ : 1 :: f. Z*n* = cof. ZC : f. ZBA = cof. ZBC :: f. Z*l* = cof. ZA : f. ZBC = cof. ZBA; and as f. CZ : 1 :: f. Z*l* = cof. ZA : f. ZCB = cof. ZCA :: f. Z*m* = cof. BZ : f. ZCA = cof. ZCB.

Now, the velocity z in AB reduced into the direction of the great circle ZA is $= z \times$ cof. ZAB $= \frac{z \times \text{cof. ZB}}{\text{f. ZA}}$, and the velocity y in the circle CA reduced into the direction of the great circle ZA $= y \times$ cof. ZAC $= \frac{y \times \text{cof. ZC}}{\text{f. ZA}}$, but in a contrary ſenſe to the former; conſequently the velocity of the point A along the great circle AZ in abſolute ſpace, that is, the velocity with which A approaches the

the fixed point Z muſt be $= \frac{z \times \text{cof. ZB} - y \times \text{cof. ZC}}{\text{ſ. ZA}}$; in like manner is found $\frac{x \times \text{cof. ZC} - z \times \text{cof. ZA}}{\text{ſ. ZB}}$ the velocity of B along BZ, and $\frac{y \times \text{cof. ZA} - x \times \text{cof. ZB}}{\text{ſ. ZC}}$ = that of C along CZ in abſolute ſpace. But the fluxions of the arcs ZA, ZB, and ZC, are $\frac{\text{cof. ZA}}{\text{ſ. ZA}}$, $\frac{\text{cof. ZB}}{\text{ſ. ZB}}$, and $\frac{\text{cof. ZC}}{\text{ſ. ZC}}$, reſpectively, which divided by their correſpondent velocities, give the fluxion of the time, that is, $\dot{t} = \frac{\text{cof. ZA}}{z \times \text{cof. ZB} - y \times \text{cof. ZC}} = \frac{a^2\dot{x}}{zb^2y - yc^2z}$ (above found) $=$ $\frac{\text{cof. ZB}}{x \times \text{cof. ZC} - z \times \text{cof. ZA}} = \frac{b^2\dot{y}}{xc^2z - za^2x} = \frac{\text{cof. ZC}}{y \times \text{cof. ZA} - x \times \text{cof. ZB}} =$ $\frac{c^2\dot{z}}{ya^2x - xb^2y}$; from which ſix-fold equation, it is evident, by inſpection only, that if m = any conſtant quantity whatever, and ma^2x = cof. ZA, mb^2y = cof. ZB, and mc^2z = cof. ZC, all the conditions thereof will be anſwered. Then, ſince cof. ZA^2 + cof. ZB^2 + cof. $\text{ZC}^2 = 1$, its equal $m^2a^4x^2 + m^2b^4y^2 + m^2c^4z^2$ muſt alſo be $= 1$: but from the former part of the proceſs $a^4x^2 + b^4y^2 + c^4z^2 = a^4\mathfrak{A}^2 + b^4\mathfrak{B}^2 + c^4\mathfrak{C}^2$; therefore $m = \frac{1}{\sqrt{a^4\mathfrak{A}^2 + b^4\mathfrak{B}^2 + c^4\mathfrak{C}^2}}$ a conſtant quantity; and ſ. $\text{AZ}^2 = 1 - \text{cof. AZ}^2$ = cof. BZ^2 + cof. $\text{CZ}^2 = 1 - m^2a^4x^2 = m^2b^4y^2 + m^2c^4z^2$, ſ. $\text{BZ}^2 = 1 - \text{cof. BZ}^2 = 1 - m^2b^4y^2 = m^2a^4x^2 + m^2c^4z^2$, and ſ. $\text{CZ}^2 = 1 - m^2c^4z^2 = m^2a^4x^2 + m^2b^4y^2$; and, from above, the velocities with which A, B, and C, approach Z are reſpectively $\frac{\sqrt{b^2 - c^2} \times yz}{\sqrt{b^4y^2 + c^4z^2}}$, $\frac{\sqrt{c^2 - a^2} \times xz}{\sqrt{a^4x^2 + c^4z^2}}$, and $\frac{\sqrt{a^2 - b^2} \times xy}{\sqrt{a^4x^2 + b^4y^2}}$; but as a is ſuppoſed greater than c, $c^2 - a^2$ is negative, and the velocity therefore in a contrary ſenſe, conſequently the poles A and C muſt approach Z, whilſt B recedes from it. The reſpective velocities

ties of the points A, B, and C, in directions perpendicular to ZA, ZB, and ZC, being computed in like manner are $\frac{z \times \text{cof. ZC} + y \times \text{cof. ZB}}{\text{f. ZA}}$, $\frac{z \times \text{cof. ZC} + x \times \text{cof. ZA}}{\text{f. ZB}}$, and $\frac{x \times \text{cof. ZA} + y \times \text{cof. ZB}}{\text{f. ZC}}$, or $\frac{c^2z^2 + b^2y^2}{\sqrt{b^4y^2 + c^4z^2}}$, $\frac{c^2z^2 + a^2x^2}{\sqrt{c^4z^2 + a^2x^2}}$, and $\frac{a^2x^2 + b^2y^2}{\sqrt{a^2x^2 + b^4y^2}}$, and if each of the ſquares of theſe be added to each correſpondent ſquare of the three former, the reſulting ſums will be $z^2 + y^2$, $z^2 + x^2$, and $x^2 + y^2$, which are the ſquares of the abſolute velocities of the poles A, B, and C, along their own proper tracks in abſolute ſpace, the operation thus proving itſelf. Hence we gain a clear idea of the motion of the body, during the time that the octant ABC takes in paſſing under Z, beginning at ſome point V in CB (or in AB as the caſe may happen) and ending at ſome point W in CA; that is, the point Z enters the octant when V touches Z, and quits it at W, the motion of the body or ſpherical ſurface that revolves with it under Z, being in the ſenſe from W towards V; that is, W approaching the fixed point Z whilſt V recedes from it. And ſince both the directions and velocities of the poles A, B, and C, in abſolute ſpace are given above, their tracks alſo may be determined by means of quadratures, as will be ſhewn hereafter. Again, the track VZW, on the moving ſpherical ſurface, which always paſſes under, or, ſome point of which, always touches Z as the body revolves; and the velocity with which it paſſes under it in every poſition may hence be determined. Thus, from the equation above found for the value of z, is eaſily obtained cof. $\text{CZ}^2 = m^2c^4z^2 = \frac{c^2 \times \overline{a^2 - b^2}}{a^2 \times b^2 - c^2} \times$ cof. $\text{AZ}^2 - \frac{m^2c^2a^2 \times \overline{a^2 - b^2}}{b^2 - c^2} \times \mathfrak{A}^2 + m^2c^4\mathfrak{C}^2$, the equation of the curve VZW upon the moving ſpherical ſurface, which will alſo be found to be

be the equation of the curve when orthographically projected upon the plane of the great circle CA. For let the ſphere be thus projected, then the quadrants AB, BC (fig. 9.) will be projected into the right lines BA, BC, and if Z be the projected place of the fixed point at any inſtant, let fall the right line ZX perpendicular to BC; then, by the nature of the projection $ZX = \text{cof. } AZ$, and $BX = \text{cof. } CZ$, and if $\frac{a^2}{b^2-c^2} = A$, $\frac{b^2}{a^2-c^2} = B$, and $\frac{c^2}{a^2-b^2} = C$, the above equation becomes $BX^2 = \frac{c^4 A}{a^4 C} \times ZX^2 - \frac{m^2 c^4 A \mathfrak{A}^2}{C} + m^2 c^4 \mathfrak{C}^2$, and $ZX^2 = \frac{a^4 C}{c^4 A} \times (BX^2 + \frac{m^2 c^4 A \mathfrak{A}^2}{C} - m^2 c^4 \mathfrak{C}^2)$ the projected track therefore upon the plane is an hyperbola, whoſe centre is B, abſciſſa BX, and ordinate ZX, and taking $ZX = 0$, $BX = mc^2 \sqrt{\mathfrak{C}^2 - \frac{A\mathfrak{A}^2}{C}}$ = the diſtance from B at which the curve cuts BC, and is therefore the ſemi-tranſverſe axis of the hyperbola. But this is only poſſible whilſt $C\mathfrak{C}^2$ is greater than $A\mathfrak{A}^2$; for if $C\mathfrak{C}^2 = A\mathfrak{A}^2$, $XZ = BX \times \frac{a^2}{c^2}\sqrt{\frac{C}{A}}$, the projected track is a right line BU, and the real one a great circle of the ſphere paſſing through B. If $A\mathfrak{A}^2$ be greater than $C\mathfrak{C}^2$ the track will no longer cut CB, but muſt cut BA, and BU will in both caſes be an aſymptote to the projected track. Since the track in all caſes croſſes the great circle CA, and we are at liberty to ſuppoſe the motion to begin at what point thereof we pleaſe, it may be ſuppoſed to commence where the track croſſes CA, and where, of conſequence, the velocity along CA is then $= 0$; we may therefore take the aſſumed quantity $\mathfrak{B} = 0$, and ſtill all the conditions of the problem be fulfilled, the expreſſions thus becoming more ſimple,

for then $\frac{1}{m^2} = a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2$ and $A\mathfrak{A}^2 - \frac{A \times \text{cof. } AZ^2}{m^2a^4} = \frac{B \times \text{cof. } BZ^2}{m^2b^4} = C\mathfrak{C}^2 - \frac{C \times \text{cof. } CZ^2}{m^2c^4}$.

Suppoſe W to be the point of CA and V that of CB which comes under Z; then at W cof. $BZ = o$, and cof. $AZ = ma^2\mathfrak{A} = $ ſ. CW; and at V, cof. $AZ = o$, and cof. $BZ = mb^2\mathfrak{A}\sqrt{\frac{A}{B}} =$ ſ. $CV =$ ſ. $CW \times \frac{b^2}{a^2}\sqrt{\frac{A}{B}}$; CV and CW being a kind of ſemi-tranſverſe and ſemiconjugate axes to the elliptic track on the ſpherical ſurface that paſſes under Z. And the gnomonical projection of the track on a plane touching the ſphere at C, or the orthographical on the plane of the great circle BA (fig. 10.) becomes known; for here $YZ =$ cof. $BZ = CX$; $CY = XZ =$ cof. AZ, and $A\mathfrak{A}^2 - CY^2 \times \frac{A}{m^2a^4} = ZY^2 \times \frac{B}{m^2b^4}$ is the equation of the curve VZW which is the projection of the track on this plane, being an ellipſis whoſe ſemi-axes are ſ. CV and ſ. CW or $mb^2\mathfrak{A}\sqrt{\frac{A}{B}}$ and $ma^2\mathfrak{A}$, becauſe $\frac{a^4B}{b^4A} \times ZY^2 = m^2a^4\mathfrak{A}^2 - CY^2$. Moreover, the perpendicular to the plane of the projection from Z on the plane to Z on the ſpherical ſurface itſelf $=$ cof. $CZ = \sqrt{m^2c^4\mathfrak{C}^2 - \frac{c^4B}{b^4C} \times ZY^2} = \sqrt{1 - CZ^2} = \sqrt{1 - ZX^2 - ZY^2}$; and the fluxion of the track at Z upon the ſpherical ſurface $= \sqrt{\text{cof. } A\dot{Z}^2 + \text{cof. } B\dot{Z}^2 + \text{cof. } C\dot{Z}^2} = m\sqrt{a^4\dot{x}^2 + b^4\dot{y}^2 + c^4\dot{z}^2}$, and ſince $\dot{t} = \frac{A\dot{x}}{yz} = -\frac{B\dot{y}}{zx} = \frac{C\dot{z}}{xy}$, we thence obtain $\dot{x}^2 = \frac{B^2y^2\dot{y}^2}{A^2x^2}$, $\dot{z}^2 = \frac{B^2y^2\dot{y}^2}{C^2z^2}$, and the fluxion of the track $= m\dot{y}\sqrt{\frac{a^4B^2y^2}{A^2x^2} + b^4 + \frac{c^4B^2y^2}{C^2z^2}}$, which divided by $\dot{t}$ gives $m\sqrt{\frac{a^4y^2z^2}{A^2} + \frac{b^4z^2x^2}{B^2} + \frac{c^4y^2x^2}{C^2}} =$ the velocity with

with which the track paſſes under Z, but $z^2 = \mathfrak{C}^2 - \frac{B}{C} \times y^2$, and $x^2 = \mathfrak{A}^2 - \frac{By^2}{A}$, alſo $z^2x^2 = \frac{B^2y^4}{AC} - \frac{B\mathfrak{A}^2}{C} + \frac{B\mathfrak{C}^2}{A} \times y^2 + \mathfrak{A}^2\mathfrak{C}^2$, which ſubſtituted for their equals give the velocity $= m\sqrt{-\frac{Ba^4y^4}{CA^2} + \frac{a^4y^2\mathfrak{C}^2}{A^2} + \frac{b^4y^4}{AC} - \frac{b^4y^2\mathfrak{A}^2}{BC} - \frac{b^4y^2\mathfrak{C}^2}{AB} + \frac{b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2} - \frac{Bc^4y^4}{AC^2} + \frac{c^4y^2\mathfrak{A}^2}{C^2}} = m\sqrt{\frac{b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2} - \frac{c^4y^2\mathfrak{C}^2}{AC} - \frac{a^4y^2\mathfrak{A}^2}{AC}} = \sqrt{\frac{m^2b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2} - \frac{y^2}{AC}}$, becauſe $\frac{b^4}{AC} - \frac{Ba^4}{A^2C^2} - \frac{Bc^4}{C^2A} = \frac{B}{AC} \times \left(\frac{b^4}{B} - \frac{a^4}{A} - \frac{c^4}{C}\right) = \frac{B}{AC} \times (b^2 \times \overline{a^2 - c^2} - a^2 \times \overline{b^2 - c^2} - c^2 \times \overline{a^2 - b^2}) = 0$, $\frac{a^4}{A^2} - \frac{b^4}{AB} = -\frac{c^4}{AC}$ and $m^2a^4\mathfrak{A}^2 + m^2c^4\mathfrak{C}^2 = 1$. Now, ſuppoſing as above, the motion to begin when W is under Z and $y = 0$, the track muſt croſs CA at right angles, and with a velocity under $Z = \overline{a^2 - c^2} \times m\mathfrak{A}\mathfrak{C} = \frac{\overline{a^2 - c^2} \times \mathfrak{A}\mathfrak{C}}{\sqrt{a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2}}$ that velocity being then the ſwifteſt poſſible, $\mathfrak{A}$, $\mathfrak{C}$, and $\sqrt{\mathfrak{A}^2 + \mathfrak{C}^2}$ being the then velocities of the poles C, A, and B, along their proper tracks in abſolute ſpace, the velocity x being then $= \mathfrak{A}$ and $z = \mathfrak{C}$, which are their greateſt values; and then Z becoming without the octant ABC, the velocity y muſt be negative or in a contrary ſenſe to what it would be if Z were within the octant; that is, ſince within the octant, y, as we have ſeen, is in the ſenſe from C towards A, it muſt now be in the ſenſe from A towards C; x and z ſtill continuing to be in the ſame ſenſe as if Z were within the octant, till the great circle BCV′ comes under Z which then touches V′, and conſequently $x = 0$, $y^2 = \frac{A\mathfrak{A}^2}{B}$, $z = \mathfrak{C}^2 - \frac{A\mathfrak{A}^2}{C}$ and $\sqrt{\frac{m^2b^2\mathfrak{A}^2\mathfrak{C}^2}{B^2} - \frac{\mathfrak{A}^2}{BC}} = \frac{ma\mathfrak{A}}{\sqrt{ABC}}\sqrt{C\mathfrak{C}^2 - A\mathfrak{A}^2}$ = the velocity of the track under Z, which is then the ſloweſt, the correſpondent velocities of the poles A, B, and C, along

their own proper tracks in abſolute ſpace being then $\sqrt{\mathfrak{C}^2+\frac{A\mathfrak{A}^2}{B}-\frac{A\mathfrak{B}^2}{C}}$, $\sqrt{\mathfrak{C}^2-\frac{A\mathfrak{B}^2}{C}}$, and $\mathfrak{A}\sqrt{\frac{A}{B}}$. And when V′ with the above found velocity has paſſed under Z, then the velocity x becomes negative; therefore, whilſt the point Z is within the angle formed by AC and BC produced beyond C both y and x are negative, till the great circle BC again croſſing under Z at W′, y is again $=0$, and the velocity of the track under Z the ſame as when W was under it, the correſponding velocities of the poles of the permanent axes being the ſame alſo; after which y will again become poſitive, x ſtill continuing negative during the time that Z is within the angle BCW′, till it again croſſes BC at V, and x is again $=0$, and the velocities of the track and permanent poles the ſame as when V′ croſſed under Z; afterwards the point Z being within the octant ABC, the velocities x, y, and z,, will be all poſitive till W again comes under Z, and another revolution under Z begins, *and ſo on for ever*. Moreover, the track being ſuppoſed to croſs CA and CB, when either W or W′ is under Z, the velocity $\sqrt{\mathfrak{A}^2+\mathfrak{C}^2}$ of the pole B is the greateſt poſſible, being then $=$the greateſt velocity that the ſpherical ſurface any where has or can have; and when V and V′ are under Z, $\sqrt{\mathfrak{C}^2+\frac{A\mathfrak{A}^2}{B}-\frac{A\mathfrak{B}^2}{C}}=$the velocity of the pole A is the ſwifteſt which it can have, being then $=$the greateſt *velocity* which the ſpherical ſurface any where has at that inſtant, ſuch *velocity* of the ſurface being then the *leaſt* poſſible.

Moreover, ſuppoſing ſtill the motion to begin when $y=0$, and $\mathfrak{B}=0$, $\dot{t}=-\frac{B\dot{y}}{\dot{x}z}=-\frac{B\dot{y}}{\sqrt{\mathfrak{A}^2-\frac{By^2}{A}}\sqrt{\mathfrak{C}^2-\frac{By^2}{C}}}=-\frac{\dot{y}\sqrt{AC}}{\sqrt{\frac{A\mathfrak{A}^2}{B}-y^2}\sqrt{\frac{C\mathfrak{C}^2}{B}-y^2}}$;

let

let $y^2 = \frac{ACu}{B}$ or $y = u^{\frac{1}{2}}\sqrt{\frac{AC}{B}}$, $\dot{y} = \frac{1}{2}\dot{u}\sqrt{\frac{AC}{Bu}}$, and $\dot{t} = \frac{AC}{\sqrt{B}} \times$

$\frac{-\dot{u}}{2u^{\frac{1}{2}}\sqrt{\frac{A\mathfrak{A}^2}{B} - \frac{ACu}{B}}\sqrt{\frac{C\mathfrak{C}^2}{B} - \frac{ACu}{B}}} = \frac{B^{\frac{1}{2}}}{2} \times \frac{-\dot{u}}{u^{\frac{1}{2}}\sqrt{\frac{\mathfrak{A}^2}{C} - u}\sqrt{\frac{\mathfrak{C}^2}{A} - u}}$; which here naturally divides into three forms or cafes, 1ft, $\frac{B^{\frac{1}{2}}}{2} \times$

$\frac{-\dot{u}}{\sqrt{\frac{\mathfrak{A}^2 u}{C} - u^2}\sqrt{\frac{\mathfrak{C}^2}{A} - u}}$; 2dly, $\frac{B^{\frac{1}{2}}}{2} \times \frac{-\dot{u}}{\sqrt{\frac{\mathfrak{C}^2 u}{A} - u^2}\sqrt{\frac{\mathfrak{A}^2}{C} - u}}$; 3dly, when

$\frac{\mathfrak{A}^2}{C} = \frac{\mathfrak{C}^2}{A}$, it is $\frac{B^{\frac{1}{2}}}{2} \times \frac{-\dot{u}}{\frac{\mathfrak{A}^2 u^{\frac{1}{2}}}{C} - u^{\frac{3}{2}}} = -\frac{\dot{y}\sqrt{AC}}{\frac{A\mathfrak{A}^2}{B} - y^2}$; which laft is of an eafy and known form; and the fluents of the two former may be found by help of the arcs of the conic fections; or otherwife, by the following contrivance.

Suppofe a bar of metal, or other fuch like body, whofe centre of ofcillation is H (fig. 11.) to revolve at the earth's furface in a vertical plane without refiftance about the centre C, and that it is impelled from the loweft point S with a velocity equal to that which would be acquired by an heavy body in falling freely by the force of uniform gravity through the height k, that is, if $2g$ = the force of gravity, fuppofe it impelled from S with a velocity $2\sqrt{gk}$ up the femicircle SMH, whofe radius $CS = CH = CM = r$; then, MV being parallel to the horizon, and $SV = u$; its velocity at M muft be $2\sqrt{gk - gu}$, and the fluxion of the arch $\dot{MS} = \dot{MH} = \frac{-r \times \dot{HV}}{MV} = \frac{r\dot{u}}{\sqrt{2ru - u^2}}$,

and the time of defcribing $\dot{SM} = \frac{r}{2} \times \frac{-\dot{u}}{g^{\frac{1}{2}}\sqrt{2ru - u^2}\sqrt{k - u}}$ becaufe the velocity diminifhes as SV increafes, this fluxion compared

with $\dot{t} = \frac{B^{\frac{1}{2}}}{2} \times \frac{-\dot{u}}{\sqrt{\frac{\mathfrak{A}^2 u}{C} - u^2}\sqrt{\frac{\mathfrak{C}^2}{A} - u}}$, we have $2r = \frac{C}{\mathfrak{A}^2}$, $k = \frac{}{C^2}$; if therefore $\frac{\mathfrak{A}^2}{2C} = CH$, we have, as $\frac{\mathfrak{A}^2}{2g^{\frac{1}{2}}C}$: the fluxion of the time of the bar's defcribing SM :: $B^{\frac{1}{2}}$: $\dot{t}$, that is, $\frac{\mathfrak{A}^2}{2C} : \sqrt{Bg} :: \frac{-r\dot{u}}{2g^{\frac{1}{2}}\sqrt{2ru - u^2}\sqrt{k-u}} : \dot{t}$; but the velocity at $H = 2g^{\frac{1}{2}}\sqrt{k - 2r} = 2g^{\frac{1}{2}}\sqrt{\frac{\mathfrak{C}^2}{A} - \frac{\mathfrak{A}^2}{C}}$, if therefore $\frac{\mathfrak{C}^2}{A}$ be greater than $\frac{\mathfrak{A}^2}{C}$ (which may be called the firft cafe) the bar will make whole revolutions round the centre C, and its velocity at H = that acquired by an heavy body in falling through the height $\sqrt{\frac{\mathfrak{C}^2}{A} - \frac{\mathfrak{A}^2}{C}}$, and at S the arch MH = the femicircle. Now, when $y = o$, that is, when W or W′ is under Z, $u = o$, SV = o, and when $u = 2r = \frac{\mathfrak{A}^2}{C} = SH$, then $y^2 = \frac{A\mathfrak{A}^2}{B}$ which is the value of y^2 at V and V′ above, the afcent therefore of the bar from S to H in the femicircle correfponds to the motion of the body during the time that the quadrant of the track beginning at W and ending at V′ paffes under Z, and the fluxions of the times being to one another as $\frac{\mathfrak{A}^2}{2Cg^{\frac{1}{2}}} : B^{\frac{1}{2}}$, the times muft be in the fame ratio, confequently, as $\frac{\mathfrak{A}^2}{2C} : \sqrt{Bg}$ :: the time of two revolutions of the bar : the time of one revolution of the track WV′W′V under Z.

But if, as in cafe fecond, $\frac{\mathfrak{A}^2}{C}$ be greater than $\frac{\mathfrak{C}^2}{A}$, and r be ftill $= \frac{\mathfrak{A}^2}{2C}$ the bar can proceed no higher than till k = that height = $\frac{\mathfrak{C}^2}{A}$, its velocity at S being $= 2g\sqrt{\frac{\mathfrak{C}^2}{A}}$, when $u = o$ and y and SV $= o$;

$=0$; and when $u=\frac{\mathfrak{C}^2}{A}$, $y^2=\frac{C\mathfrak{C}^2}{B}$ which is its value when $z=0$, as it ought to be, the track in this caſe, that is, when $A\mathfrak{A}^2$ is greater than $C\mathfrak{C}^2$, croſſing AC and AB; the bar in this caſe making only oſcillations and not revolutions. But if r now be made $=\frac{\mathfrak{C}^2}{2A}$ inſtead of $\frac{\mathfrak{A}^2}{2C}$, the bar will ſtill make whole revolutions and as $\frac{\mathfrak{C}^2}{2A} : \sqrt{Bg}$ :: the time of two whole revolutions of the bar whoſe centre of oſcillation is at $\frac{\mathfrak{C}^2}{2A}$ diſtance from C : the time of one revolution of the body under Z.

Theſe caſes may be otherwiſe reſolved by finding the length $SC=r$, ſuch that the bar may make two revolutions or oſcillations whilſt the body makes one; thus, let SV, inſtead of being $=u$, be in a conſtant ratio to it, or $SV=lu$, and

$$\dot{t}=\frac{r}{2}\times\frac{l\dot{u}}{g^{\frac{1}{2}}\sqrt{2rlu-l^2u^2}\sqrt{k-lu}}=\frac{B^{\frac{1}{2}}}{2}\times\frac{\dot{u}}{\sqrt{\frac{\mathfrak{A}^2u}{C}-u^2}\sqrt{\frac{\mathfrak{C}^2}{A}-u}}=\frac{B^{\frac{1}{2}}}{2}\times\frac{\dot{u}l\sqrt{l}}{\sqrt{\frac{\mathfrak{A}^2l^2u}{C}-l^2u^2}\sqrt{\frac{\mathfrak{C}^2l}{A}-lu}},$$

and comparing the homologous quantities, $\frac{l\sqrt{lB}}{2}=\frac{rl}{2g^{\frac{1}{2}}}$, $r=\sqrt{lBg}$, $2rl=\frac{\mathfrak{A}^2l^2}{C}$, $r=\frac{\mathfrak{A}^2l}{2C}=\sqrt{lBg}$, $\sqrt{l}=\frac{2C\sqrt{Bg}}{\mathfrak{A}^2}$, $l=\frac{4C^2Bg}{\mathfrak{A}^4}$, $r=\frac{2CBg}{\mathfrak{A}^2}$, $k=\frac{\mathfrak{C}^2l}{A}=\frac{4C^2Bg\mathfrak{C}^2}{A\mathfrak{A}^4}$; now, when ſuch a bar makes whole revolutions, k muſt be greater than $2r$, or $\frac{4C^2Bg\mathfrak{C}^2}{A\mathfrak{A}^4}$ than $\frac{4CBg}{\mathfrak{A}^2}$, $\frac{C\mathfrak{C}^2}{A\mathfrak{A}^2}$ than unity, and $C\mathfrak{C}^2$ than $A\mathfrak{A}^2$.

A bar therefore whoſe centre of oſcillation is $\frac{2CBg}{\mathfrak{A}^2}$ diſtant from the centre of motion, will make two whole revolutions whilſt the whole track WV′W′V moves once under Z if

if $C\mathfrak{C}^2$ be greater than $A\mathfrak{A}^2$; but if $C\mathfrak{c}^2$ be leſs than $A\mathfrak{A}^2$ it will make two whole oſcillations. In like manner it will be found, that if $SC=r=\frac{2ABg}{\mathfrak{C}^2}$, ſuch bar will make whole revolutions when $A\mathfrak{A}^2$ is greater than $C\mathfrak{C}^2$, and oſcillations when $A\mathfrak{A}^2$ is leſs than $C\mathfrak{C}^2$; and we are at liberty to make either the one ſuppoſition or the other.

Caſe 3. But if $A\mathfrak{A}^2=C\mathfrak{C}^2$, and the track that paſſes under Z be a great circle of the ſphere, then $Ax^2=Cz^2$, $\frac{x^2}{C}=\frac{z^2}{A}$, $By^2=A\mathfrak{A}^2-Ax^2=A\mathfrak{A}^2-Cz^2$, $\frac{y^2}{AC}=\frac{\mathfrak{A}^2-x^2}{BC}$, and the velocity under $Z=m\sqrt{\frac{b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2}-\overline{c^4\mathfrak{C}^2+a^4\mathfrak{A}^2}\times\frac{\mathfrak{A}^2-x^2}{BC}}=\frac{m}{BC}\sqrt{b^4 AC\mathfrak{A}^4-\overline{\mathfrak{A}^2-x^2}\times\overline{c^4AB\mathfrak{A}^2+a^4BC\mathfrak{A}^2}}=\frac{m\mathfrak{A}x}{BC}\sqrt{\overline{c^4AB+a^4BC}}=\frac{m\mathfrak{A}b^2x}{B}\sqrt{\frac{A}{C}}$, which is therefore $=0$ when $x=0$, or B is under Z, ſuppoſing that to be poſſible. But then $\dot{t}=\frac{-\dot{y}\sqrt{AC}}{\frac{A\mathfrak{A}^2}{B}-y^2}=\frac{\sqrt{CB}}{2\mathfrak{A}}\times 2\mathfrak{A}\sqrt{\frac{A}{B}}\times\frac{-\dot{y}}{\frac{A\mathfrak{A}^2}{B}-y^2}$, and $t=\frac{\sqrt{CB}}{2\mathfrak{A}}\times$ hyp. log. of $\frac{\mathfrak{A}\sqrt{\frac{A}{B}}-y}{\mathfrak{A}\sqrt{\frac{A}{B}}+y}$; therefore, when at the firſt inſtant $y=0$, to have the motion poſſible, y muſt be a negative quantity; which is agreeable to what was obſerved before, that y muſt be negative within the angle ACV'; but in this caſe Z can never come over V', for then t would be infinite. And if the motion be ſuppoſed to begin when Z is ſomewhere within the octant ABC, where y the firſt inſtant is equal to a given quantity $\mathfrak{B}$, then the fluent muſt be ſo corrected as that $t=\frac{\sqrt{CB}}{2\mathfrak{A}}\times$ hyp. log. of

$\frac{\text{R}\sqrt{\frac{A}{B}+\text{B}}}{\text{R}\sqrt{\frac{A}{B}-\text{B}}} \times \frac{\text{R}\sqrt{\frac{A}{B}-y}}{\text{R}\sqrt{\frac{A}{B}+y}}$, but the time or motion can never begin at B, nor can the pole oppoſite to B ever come under Z. And the reaſon of this is alſo evident from the nature of the motion itſelf; for theſe being poles of a permanent axis, if Z were once over one of them, it muſt always continue ſo.

Having thus determined the time, velocity, and manner, in which the ſpherical ſurface that revolves with the body paſſes under the fixed point Z, it only remains to determine the path of one of the poles as C of the permanent axes about Z in abſolute ſpace, or upon a ſpherical ſurface at reſt, but equal and concentric with that ſuppoſed to move with the body; for the path of one of theſe poles as C being found, thoſe of the other two, and indeed the path of every other invariable point of the moving ſpherical ſurface, becomes known.

Now, the velocity with which C approaches Z is found above $= \frac{\overline{a^2-b^2} \times xy}{\sqrt{a^4x^2+b^4y^2}}$, and the fluxion of the arc $\text{CZ} = \frac{\text{f. C}\dot{\text{Z}}}{\text{cof. CZ}} = -\frac{\text{cof. C}\dot{\text{Z}}}{\text{f. CZ}}$ divided by the velocity gives t, whoſe fluent is found above, and conſequently the diſtance of C from Z at the end of any time t, there is then only wanting the angle deſcribed by C about Z, correſponding to the diſtance CZ therefrom, to have the path of C about Z; which may be found by the help of quadratures as follows.

As f. ZC : velocity of C perpendicular to ZC (found above) $= \frac{x \times \text{cof. AZ} + y \times \text{cof. BZ}}{\text{f. ZC}}$ :: 1 : the angular velocity of C about $\text{Z} = \frac{x \times \text{cof. AZ} + y \times \text{cof. BZ}}{\text{f. ZC}^2}$ which velocity being multiplied by

cof.

$\frac{\text{cof. } Z\dot{C}}{y \times \text{cof. } AZ - x \times \text{cof. } BZ}$ the fluxion of the time, gives the fluxion of the angle described by C about $Z = \frac{\text{cof. } Z\dot{C}}{\text{f. } ZC^2} \times \frac{x \times \text{cof. } AZ + y \times \text{cof. } BZ}{y \times \text{cof. } AZ - x \times \text{cof. } BZ}$ $= \frac{\text{cof. } Z\dot{C}}{\text{f. } ZC^2} \times \frac{b^2 \times \text{cof. } AZ^2 + a^2 \times \text{cof. } ZB^2}{a^2 - b^2 \times \text{cof. } ZA \times \text{cof. } ZB}$, which in terms of ZC is by computation $= \frac{\text{cof. } Z\dot{C}}{\text{f. } ZC^2} \times \frac{ba^2 \times \text{f. } ZC^2 - b \times \overline{a^2 - c^2} \times s^2}{a\sqrt{b^2 - c^2} \times \overline{a^2 - c^2}\sqrt{\text{f.}CV^2 - \text{f.}ZC}\sqrt{n^2 - \text{cof.}ZC^2}}$; where s and n = the sine and cosine of CW, and $\text{f. } CV^2 = \frac{b^2 s^2}{a^2} \times \frac{a^2 - c^2}{b^2 - c^2}$. Now, this being the fluxion of the arc to radius 1, which is the measure of the angle described by C about Z in the time t; this arc in value therefore will be double the area of the sector of the circle whose radius is unity described about Z in the same time. Hence, having found a sector of a circle to radius unity, whose area is half the fluent of the above fluxion, or the fluent of half the above fluxion, the arch-line of this sector will be the measure of the required angle described by C about Z in the time t.

Let $\dot{A} = \frac{\text{cof. } Z\dot{C}}{\sqrt{n^2 - \text{cof. } ZC^2}}$, A being the arc, beginning when $n =$ cof. ZC, whose cosine $= \frac{\text{cof. } ZC}{n}$ and radius unity, and $\dot{B} = \frac{-\text{f. } Z\dot{C}}{\sqrt{\text{f. } CV^2 - \text{f. } ZC^2}}$, B being = the arc, beginning when CV = ZC, whose cosine $= \frac{\text{f. } ZC}{\text{f. } CV}$ and radius unity, and in fig. 12. take ZY such that $\frac{\text{cof. } Z\dot{C}}{\sqrt{n^2 - \text{cof. } ZC^2}} \times \frac{ba^2 \times \text{f. } ZC^2 - b \times \overline{a^2 - c^2} \times s^2}{2a\sqrt{b^2 - c^2} \times \overline{a^2 - c^2} \times \text{f.}ZC^2 \sqrt{\text{f. } CV^2 - \text{f.}ZC^2}}$ may $= \dot{A} \times \frac{ZY^2}{2}$ the fluxion of the curvilinear area described about the centre Z and bounded by the ordinate ZY, whose first value is ZG when $n =$ cof. ZC, and $A = 0$; on ZG take

ZS = 1, with which radius on the centre Z deſcribe the circle STR′ on which take ST = any value of A, and through T draw ZY = the ordinate correſponding to that value of A, and thus may points at pleaſure be found, and the curve GY conſtructed. Now, when ZC = CV, the value of $ZY = \sqrt{\frac{la^2 \times \text{f. } ZC^2 - bs^2 \times \overline{a^2 - c^2}}{a\sqrt{\overline{b^2 - c^2} \times \overline{a^2 - c^2} \times \text{f. } ZC^2 \sqrt{\text{f.}CV^2 - \text{f.}ZC^2}}}}$ is infinite, and if SR = the then value of the arc A, ZR produced will be an aſymptote to the curve GY. But to remedy this inconveniency ariſing to the conſtruction from this infinite length of the curve; produce any other radius ZR′ of the circle, till ZH = the firſt value of ZY′ $\sqrt{\frac{ba^2 \times \text{f. } ZC^2 - bs^2 \times \overline{a^2 - c^2}}{a\sqrt{\overline{b^2 - c^2} \times \overline{a^2 - c^2} \times \text{f. } ZC \times \text{cof. } ZC \sqrt{n^2 - \text{cof. } CZ^2}}}}$, when CV = ZC and the arc B = o, and taking ZY′ = any other value thereof correſponding to ſome value R″T″ of the arc B leſs than RS′ the value thereof when $n = \text{cof. } ZC$ and ZY′ infinite; and thus the curve HY may be conſtructed by points; let the conſtructions of both theſe curves GY and HY′ be continued till the value of the arc ZC in the one conſtruction be equal to that in the other; then muſt the ſum of the correſponding areas ZGY + ZHY′ be equal to the infinitely extended area formed by each curve running out towards its own aſymptote, each of theſe infinitely extended areas being equal becauſe they begin together, and are the fluents of the equal fluxions $\dot{A} \times \frac{ZY^2}{2}$ and $\dot{B} \times \frac{ZY''^2}{2}$. Equal to any value of the area ZGY, let the ſector QZR be cut off from the circle whoſe radius is unity; then the area of this ſector = half the arc RQ = the fluent of $\frac{\text{cof. } \dot{ZC}}{\sqrt{n^2 - \text{cof. } ZC^2}} \times \frac{ba^2 \times \text{f. } ZC^2 - bs^2 - \overline{a^2 - c^2}}{2a\sqrt{\overline{b^2 - c^2} \times \overline{a^2 - c^2} \times \text{f.}ZC^2 \sqrt{\text{f.}CV^2 - \text{f.}ZC^2}}}$;

and the fluent of $\frac{-\text{f. }\dot{ZC}}{\sqrt{\text{f. }CV^2 - \text{f. }ZC^2}} \times$

$\frac{ba^2 \times \text{f. }ZC^2 - bs^2 \times \overline{a^2 - c^2}}{2a\sqrt{b^2 - c^2} \times \overline{a - c^2} \times \text{f. }ZC \times \text{cof. }ZC \sqrt{n^2 - \text{cof. }ZC^2}}$ alſo = the ſector QZV = ½VQ = the fluent of $\dot{B} \times \frac{ZY''^2}{2}$, the former being that of $\dot{A} \times \frac{ZY^2}{2}$. Then, ſuppoſing ſtill the motion to begin when $y = o$, or ZC = CW, the arch QR muſt be the meaſure of the angle deſcribed by C about Z in the time t; and the whole arch RQV = the meaſure of the angle deſcribed during the time that ZC from being = CW becomes = CV, that is, during one-fourth of the time in which the track on the ſpherical ſurface makes one revolution or paſſes once under Z. Conſequently, if on ZR there be taken the right line ZC = the ſine of CW, and on CV, ZC″ = f. CV, and upon the intermediate radii as ZQ their correſpondent values of f. ZC, a curve drawn through all theſe points C, C′, C″, &c. will be the orthographical projection (upon a plane 90° from Z) of that which is the locus of C in abſolute ſpace, or upon the immoveable ſpherical ſurface; ſuch locus touching the circle whoſe radius ZC = f. CW at C, and that whoſe radius ZC″ = f. CV at C″. And the time of moving from C where ZC = f. CW to C″ where ZC″ = f. CV will be equal to that of a ſemirevolution a ſemivibration of the bar above found; and every ſucceeding part of the curve as C″, C‴, C⁗, deſcribed in the ſame or an equal time will be perfectly equal and ſimilar to C, C′, C″. If the angle CZC⁗ be a diviſor of 360°, the path will return into itſelf; if not, it will croſs itſelf ſomewhere as at C^v, and ſo on for ever.

GENERAL

GENERAL SCHOLIA.

1. Since the moving ſpherical ſurface paſſes under the fixed point Z in the ſenſe from Z towards V, and the invariable pole or point C on that ſurface moves round Z in a contrary ſenſe BCA (fig. 4. and 8.) there muſt be ſome point as O upon the ſurface which muſt be at reſt with reſpect to both theſe motions, and which point O muſt be the pole of the momentary axis, as will appear preſently; for the preceding ſolution being completed without any regard to ſuch axis, it may now be proper to deduce the properties of this axis therefrom, as by theſe means ſome *new light* may ſtill be caſt upon the motion under conſideration.

Let O (fig. 4.) be ſuch an axis, whoſe properties are conſidered in the propoſitions preceding the laſt, and let the angular velocity of the body about it $=\varepsilon$, cof. AO$=\beta$, cof BO$=\gamma$, cof. CO$=\delta$; then it has been already ſhewn, that $\varepsilon\beta=x$, $\varepsilon\gamma=y$, and $\varepsilon\delta=z$; let theſe values be ſubſtituted for x, y, and z, in the general equations of the laſt propoſition; then $\beta^2+\gamma^2+\delta^2=1$, $x^2+y^2+z^2=\varepsilon^2=\varepsilon^2\beta^2+\varepsilon^2\gamma^2+\varepsilon^2\delta^2$, and ſuppoſing ſtill the motion to begin when $y=0$, $\gamma=0$, and $\varepsilon^2=x^2+z^2=\mathfrak{A}^2+\mathfrak{C}^2=e^2$; that is, let $e=$ the angular velocity about the momentary axis when its pole O croſſes the great circle AC; then, ſince $x^2=\mathfrak{A}^2-\frac{B}{A}\times y^2$, and $z^2=\mathfrak{C}^2-\frac{By^2}{C}$, $x^2+y^2+z^2$

$$=\varepsilon^2=\mathfrak{A}^2-\frac{By^2}{A}+\mathfrak{C}^2-\frac{By^2}{C}+y^2=e^2+\varepsilon^2\gamma^2\times\overline{1-\frac{B}{A}-\frac{B}{C}}=e^2-\frac{\varepsilon^2\gamma^2}{AC}$$

(becauſe $1-\frac{B}{A}-\frac{B}{C}=-\frac{1}{AC}$), and $\varepsilon^2=\frac{e^2}{1+\frac{\gamma^2}{AC}}$, which therefore can never be conſtant whilſt γ or BO is variable, except

either $\frac{1}{A}$ or $\frac{1}{C}=0$, that is, when either $b^2=c^2$ or $a^2=b^2$. In like manner it will alſo be found, that $\varepsilon^2=\frac{e^2-\frac{\mathfrak{C}^2}{BA}}{1-\frac{\delta^2}{BA}}=\frac{e^2-\frac{\mathfrak{A}^2}{BC}}{1-\frac{\beta^2}{BC}}$; and $\delta^2=\frac{BA}{e^2-\frac{\mathfrak{A}^2}{BC}}\times\left(\frac{\mathfrak{C}^2}{BA}-\frac{\mathfrak{A}^2}{BC}+\sqrt{e^2-\frac{\mathfrak{C}^2}{BA}}\times\frac{\beta^2}{BC}\right)$, and when $\beta=0$, or the pole of the momentary axis croſſes BC, $\delta^2=\frac{BA\times\sqrt{\frac{\mathfrak{C}^2}{BA}-\frac{\mathfrak{A}^2}{BC}}}{e^2-\frac{\mathfrak{A}^2}{BC}}=\frac{\mathfrak{C}^2-\frac{A\mathfrak{A}^2}{C}}{e^2-\frac{\mathfrak{A}^2}{BC}}$, and to have this poſſible it is neceſſary that $C\mathfrak{C}^2$ be greater than $A\mathfrak{A}^2$, and it is above determined, that under the ſame limitation Z muſt alſo croſs BC.

Again, from the equation $\frac{e^2}{1+\frac{\gamma^2}{AC}}=\frac{e^2-\frac{\mathfrak{C}^2}{BA}}{1-\frac{\delta^2}{BA}}$, $e^2-\frac{e^2\delta^2}{BA}=e^2-\frac{\mathfrak{C}^2}{BA}+\frac{e^2\gamma^2}{AC}-\frac{\mathfrak{C}^2\gamma^2}{BCA^2}$, $\delta^2=\frac{\mathfrak{C}^2}{e^2}+\frac{\mathfrak{C}^2\gamma^2}{ACe^2}-\frac{B\gamma^2}{C}$, and when $\gamma=0$, or O croſſes CA, $\delta^2=\frac{\mathfrak{C}^2}{e^2}$, let $\frac{\mathfrak{C}}{e}=m$, and then $\delta^2=m^2+\frac{m^2\gamma^2}{AC}-\frac{B\gamma^2}{C}$, or $m^2-\delta^2=\frac{B\gamma^2}{C}-\frac{m^2\gamma^2}{AC}$, which is the very equation brought out by a different method in the firſt ſcholium to the ſixth propoſition above. And if $n=\frac{\mathfrak{A}}{e}=$ the coſ. of the arc of which m is the ſine, it will be found in the very ſame manner that $\beta^2=n^2+\frac{n^2\gamma^2}{AC}-\frac{B\gamma^2}{A}$. Moreover, becauſe $A\times\sqrt{\mathfrak{A}^2-x^2}=By^2=C\times\sqrt{\mathfrak{C}^2-z^2}=A\times\sqrt{\mathfrak{A}^2-\varepsilon^2\beta^2}=B\varepsilon^2\gamma^2=C\times\sqrt{\mathfrak{C}^2-\varepsilon^2\delta^2}$, $\varepsilon^2=\frac{A\mathfrak{A}^2}{B\gamma^2+A\beta^2}=\frac{C\mathfrak{C}^2}{B\gamma^2+C\delta^2}$, $\frac{A\mathfrak{A}^2}{C\mathfrak{C}^2}=\frac{An^2}{Cm^2}=\frac{B\gamma^2+A\beta^2}{B\gamma^2+C\delta^2}$, $t=-\frac{By}{xz}=-B\times\frac{\varepsilon\dot{\gamma}+\gamma\dot{\varepsilon}}{\varepsilon^2\beta\delta}=-B\times\frac{\varepsilon^2\gamma\dot{\gamma}+\gamma^2\varepsilon\dot{\varepsilon}}{\varepsilon^3\beta\gamma\delta}$, but $\varepsilon^2\times\sqrt{1+\frac{\gamma^2}{AC}}=e^2$ a conſtant quantity; therefore $\varepsilon\dot{\varepsilon}\times\sqrt{1+\frac{\gamma^2}{AC}}+$

ε^2

$\frac{\dot{\alpha}^2\gamma\dot{\gamma}}{AC}=0$, and $\dot{t}=\frac{ABC\dot{\alpha}}{\alpha^2\beta\gamma\delta}$, or $\frac{\dot{\alpha}}{\dot{t}}=\frac{\alpha^2\beta\gamma\delta}{ABC}$ = the accelerating force acting along the midcircle at 90° from O. Since, when $\gamma=0$, $y=0$, and cof. BZ = 0, the points Z and O are both upon CA at the ſame inſtant, and when $\beta=0$, $x=0$, and cof. AZ = 0, alſo when $\delta=0$, $z=0$, and cof. CZ = 0; therefore the poles Z and O both enter the octant ABC at the ſame inſtant; both, when $C\mathfrak{C}^2$ is greater than $A\mathfrak{A}^2$, croſs BC at the ſame inſtant but at different points, *viz.* Z at V where f. CV^2 = f. $CW^2 \times \frac{b^2}{a^2}\times\frac{a^2-c^2}{b^2-c^2}=\frac{\mathfrak{A}^2\times a^2b^2\times\overline{a^2-c^2}}{\overline{b^2-c^2}\times\overline{a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}}$; and O where cof. $BO^2=\gamma^2=$

$$\frac{n^2}{\frac{B}{A}-\frac{n^2}{AC}}=\frac{\mathfrak{A}^2}{\frac{B\mathfrak{C}^2}{A}+\mathfrak{A}^2-\frac{B\mathfrak{A}^2}{C}}=\frac{\mathfrak{A}^2a^2c^2\times\overline{a^2-c^2}}{\overline{b^2-c^2}\times b^2c^2\mathfrak{C}^2+a^2\mathfrak{A}^2\times\overline{b^2-c^2}\times\overline{b^2+c^2-a^2}}$$

which cannot be greater than the correſponding value of f. CV^2 above; for, ſuppoſe the contrary, and that cof. BO^2 is greater than f.VC^2, then muſt $\frac{c^2}{b^2c^2\mathfrak{C}^2+a^2\mathfrak{A}^2\times\overline{b^2+c^2-a^2}}$ be greatcr than $\frac{b^2}{a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}c^2a^4\mathfrak{A}^2+c^6\mathfrak{C}^2$ than $b^4c^2\mathfrak{C}^2+b^2a^2\mathfrak{A}^2\times\overline{b^2+c^2-a^2}$; $\mathfrak{A}^2\times\overline{c^2a^4-a^2b^4-a^2b^2c^2+a^2b^2}$ than $\overline{b^4c^2-c^6}\times\mathfrak{C}^2$, $a^2\mathfrak{A}^2\times\overline{a^2-b^2}\times\overline{c^2+b^2}$ than $c^2\mathfrak{C}^2\times\overline{b^4-c^4}$; $a^2\mathfrak{A}^2\times\overline{a^2-b^2}$ than $c^2\mathfrak{C}^2\times\overline{b^2-c^2}$, and $A\mathfrak{A}^2$ than $C\mathfrak{C}^2$ which is impoſſible whilſt Z croſſes BC, becauſe it has been proved, that then $C\mathfrak{C}^2$ is greater than $A\mathfrak{A}^2$; conſequently O croſſes BC between V and C (in fig. 8.) and both O and Z quit the octant ABC at the ſame inſtant; Z at W, and O between W and C, at the point where $\gamma^2=0$, and $\beta^2=n^2=\frac{\mathfrak{A}^2}{e^2}$, and, as will be more fully ſhewn, a great circle drawn from O to Z being always perpendicular to the track VZW. In the very ſame manner it may be ſhewn, that when $A\mathfrak{A}^2$ is greater than $C\mathfrak{C}^2$, and the track which paſſes under Z

croſſes BA, both O and Z ſtill enter the octant ABC together, both paſs over it in the ſame time, and both quit it or croſs CA together; but in this caſe the track for Z upon the moving ſurface is leſs than, or within, that of O, Z croſſing BA at a point nearer to A than that where O croſſes it; and O in both theſe caſes ſhifts its place on the moving ſpherical ſurface making one revolution in the time that the whole curve WV′W′V takes in paſſing under Z; both curves being ſuch that in the caſes above deſcibed where the projection of WV′W′V is a conic ſection, that of the track of O projected upon the ſame plane will be a conic ſection alſo, that is, where it is ſhewn above that the projecton of WV′W′V is an hyperbola, that of the track O will be an hyperbola, and an ellipſis where that of the other is an ellipſis.

And when $A\mathfrak{A}^2 = C\mathfrak{C}^2$ or $\mathfrak{C}^2 : \mathfrak{A}^2 :: A : C :: \delta^2 : \beta^2$, the track of O as well as Z is a great circle of the ſphere, ſince $\frac{\mathfrak{A}^2}{\mathfrak{C}^2} = \frac{C}{A}$, and ſ. $CQ^2 = \frac{C}{A} \times$ ſ. AQ^2 when O croſſes CA at Q (fig. 4.); and when Z croſſes CA coſ. $AZ^2 =$ ſ. $CW^2 = \frac{a^4\mathfrak{A}^2}{a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2}$, and ſ. $CQ^2 = \frac{Cm^2}{A} = \frac{C}{A} \times \frac{\mathfrak{C}^2}{\mathfrak{A}^2+\mathfrak{C}^2} = \frac{C}{C+A}$, and ſ. $CW^2 = \frac{1}{1+\frac{c^4 A}{a^4 C}} = \frac{C}{C+\frac{a^4 A}{a^4}}$, conſequently, a^2 being, by hypotheſis, greater than c^2, the ſine of CW muſt be greater than ſ. CQ or than ſ. CO when O croſſes CA; and therefore the point where O croſſes CA muſt be nearer C than the point where Z croſſes it the ſame inſtant, in the caſe where both the tracks are great circles of the ſphere, paſſing through the ſame point B.

2. It is now well known, that the *momentum* of *inertia* of the body round the axis whoſe pole is O is $= Ma^2\beta^2 + Mb^2\gamma^2 + Mc^2\delta^2$, and if this be drawn into ε^2, the product $M\varepsilon^2 \times (a^2\beta^2 +$

b^2,

$b^2\gamma^2+c^2\delta^2)=M\times(a^2x^2+b^2y^2+c^2z^2)=$ the whole *vis viva* of the body, or becauſe radius is unity, it is = the centrifugal motive force of the body round the natural or momentary axis, which being equal to the ſum of Ma^2x^2, Mb^2y^2, and Mc^2z^2, thoſe round the three permanent ones, and being above proved to be a conſtant quantity, the perturbating motive forces $M\times\overline{b^2-c^2}\times yz$, $M\times\overline{c^2-a^2}\times zx$, and $M\times\overline{a^2-b^2}=xy$, above found, cannot alter the *vis viva*, or whole motive force of the body along the midcircle, or that which is 90° from O. But, for a more particular proof of this, let theſe oblique perturbating motive forces be reſolved into three others acting in the direction of the midcircle; the firſt ſo reſolved being $=M\times\overline{b^2-c^2}\times yz\beta=M\varepsilon^2\beta\gamma\delta\times\overline{b^2-c^2}$, the ſecond $=M\varepsilon^2\beta\gamma\delta\times\overline{c^2-a^2}$, and the third $=M\varepsilon^2\beta\gamma\delta\times\overline{a^2-b^2}$; their ſum $M\varepsilon^2\beta\gamma\delta\times(b^2-c^2+c^2-a^2+a^2-b^2)$ being $=0$, ſhews that there is no motive force in the direction of the midcircle ariſing from them, wherefore that along the midcircle muſt remain unaltered. But, though there is no perturbating motive force in the direction of the midcircle, there is nevertheleſs an accelerative one acting along it; for the three perturbating accelerative forces round the three permanent axes being $\frac{b^2-c^2}{a^2}\times yz$, $\frac{c^2-a^2}{b^2}\times zx$, and $\frac{a^2-b^2}{c^2}\times xy$, theſe being reſolved into the direction of the midcircle, their ſum $\varepsilon^2\beta\gamma\delta\times\left(\frac{b^2-c^2}{a^2}+\frac{c^2-a^2}{b^2}+\frac{a^2-b^2}{c^2}\right)=\varepsilon^2\beta\gamma\delta\times\left(\frac{1}{A}-\frac{1}{B}+\frac{1}{C}\right)$ will not be $=0$, but to $\frac{\varepsilon^2\beta\gamma\delta}{ABC}$ which is the value of $\frac{\dot{\varepsilon}}{\dot{t}}$ found in the preceding ſcholium, and by the general properties of all ſpherical motion as proved in the fourth propoſition above is the accelerating force acting there.

This

This matter M. EULER conſiders in a ſomewhat different light, by finding the *initial axis*, or that about which, if the body were perfectly at reſt, it would be firſt urged to turn by accelerating forces acting upon it; and from Scholium 1. Prop. IV. above it appears, that if the body were at reſt, and acted upon by three external accelerating forces $\frac{\dot{x}}{t}$, $\frac{\dot{y}}{t}$, and $\frac{\dot{z}}{t}$, it would be urged to turn the firſt inſtant about ſome axis whoſe pole is E by a ſingle force $= \frac{\sqrt{\dot{x}^2+\dot{y}^2+\dot{z}^2}}{t}$, ſuch that the five forces, $\frac{\sqrt{\dot{x}^2+\dot{y}^2+\dot{z}^2}}{t}$, $\frac{\dot{x}}{t}$, $\frac{\dot{y}}{t}$, $\frac{\dot{z}}{t}$ and $\frac{\dot{s}}{t}$ will be reſpectively as radius, cof. EA, cof. EB, cof. EC, and cof. EO, or $\frac{\dot{x}}{t \times \text{cof. EA}} = \frac{\dot{y}}{t \times \text{cof. EB}} = \frac{\dot{z}}{t \times \text{cof. EC}} = \frac{\dot{s}}{t \times \text{cof. EO}}$, and ſince when the body is in motion, and that motion diſturbed by the unequal action of its own particles which generates accelerating forces, ſuch forces conſidered ſimply in themſelves muſt ſtill have the ſame tendency to turn the body about ſome axis whoſe pole is E different from that whoſe pole is O, and ſuch that the above equation may ſtill obtain, and if the above-found values of $\frac{\dot{x}}{t}$, $\frac{\dot{y}}{t}$, and $\frac{\dot{z}}{t}$, be ſubſtituted therein, by means of a *calculus* ſo inſtituted, the value of $\dot{s}$, and conſequently s will come out the very ſame as by the preceding methods.

3. It ſtill remains to be ſhewn, that the point Z now determined has the properties ſhewn to be requiſite in the fifth Propoſition above, *viz.* that it is at reſt in abſolute ſpace, and therefore at reſt both with reſpect to the motion of the ſpherical ſurface, and to the velocity with which O the pole of the momentary axis ſhifts its place. Now, by Scholium 1. Prop. IV.

IV. the momentary pole ſhifts its place along its own track with a velocity $=\frac{\sqrt{\dot\beta^2+\dot\gamma^2+\dot\delta^2}}{i}$; and if Z be ſuch as that the great circle OZ (fig. 4.) may always be perpendicular to the track WV′W′V (fig. 8.) that paſſes under Z, which it muſt be if O be the pole of motion; then as $1 : s^2 ::$ ſ. $OZ^2 =$ coſ. OY^2 : the ſquare of the velocity of the track under $Z = \frac{m^2b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2} - \frac{y^2}{AC}$, hence ſ. $OZ^2 = \frac{m^2b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2s^2} - \frac{y^2}{ACs^2}$, coſ. $OZ^2 = 1 + \frac{y^2}{ACs^2} - \frac{m^2b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2s^2} = 1 + \frac{\gamma^2}{AC} - \frac{m^2b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2s^2} = \frac{e^2}{s^2} - \frac{m^2b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2s^2} = \frac{\overline{a^2\mathfrak{A}^2+c^2\mathfrak{C}^2}^2}{s^2\times a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}$, and coſ. $ZO = \frac{a^2\mathfrak{A}^2+c^2\mathfrak{C}^2}{s\sqrt{a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}} =$ ſ. OY; this, then, is the value of coſ. ZO deduced from the ſuppoſition that it is always perpendicular to the track upon the moving ſpherical ſurface which paſſes under Z at reſt; and if this be found to agree with the value thereof computed by trigonometry, it will prove the legitimacy of that ſuppoſition, and that it is the true value ſuch as that O ſhall be always the pole of the momentary axis and Z at reſt in abſolute ſpace. Produce BZ (fig. 4.) till it cuts AC perpendicularly at q; then it is before found, that the ſine and coſine of BZ are $m\sqrt{a^4x^2+c^4z^2}$ and mb^2y, thoſe of CZ $m\sqrt{a^4x^2+b^4y^2}$ and mc^2z, and as ſ. BZ : 1 :: coſ. CZ : ſ. $AQ = \frac{c^2z}{\sqrt{a^4x^2+c^4z^2}}$, and coſ. $Aq = \frac{a^2x}{\sqrt{a^4x^2+c^4z^2}}$, $\frac{y}{s} =$ coſ. BO, $\frac{\sqrt{x^2+z^2}}{s} =$ ſ. BO, $\frac{z}{s} =$ coſ. CO, $\frac{\sqrt{x^2+y^2}}{s} =$ ſ. CO as ſ. BO : 1 :; coſ. CO : ſ. $QA = \frac{z}{\sqrt{x^2+z^2}}$, coſ. $QA = \frac{x}{\sqrt{x^2+z^2}}$, and the arch Qq $= AQ - Aq =$ the meaſure of the angle OBZ, and coſ. $Qq =$

$\frac{a^2x^2+c^2z^2}{\sqrt{x^2+z^2}\sqrt{a^4x^2+c^4z^2}}$, hence cof. OZ = f. BO × f. BZ × cof. Qq + cof. BO × cof. BZ = $\frac{\sqrt{x^2+z^2}}{s} \times m\sqrt{a^4x^2+c^4z^2} \times$ cof. $Qq + \frac{1}{s} \times mb^2y = \frac{m}{s} \times (a^2x^2+c^2z^2+b^2y^2) = \frac{a^2\mathfrak{A}^2+c^2\mathfrak{C}^2}{s\sqrt{a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}}$, the very ſame as before, proving the truth of the ſuppoſition. And by the nature of the motion as rad. = 1 : s :: cof. OZ = f. OY : $\frac{a^2\mathfrak{A}^2+c^2\mathfrak{C}^2}{\sqrt{a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}}$ = the velocity of the moving ſpherical ſurface at Y. Now, it does not appear, that there is any one point upon the varying great circle ZOY, which (in general) continues always the ſame or invariable upon the moving ſpherical ſurface, to find therefore the path of O about Z in abſolute ſpace, it is neceſſary to conſider, that the point O, whereſoever upon the ſpherical ſurface it is found, can have but one proper direction of motion and velocity with which it ſhifts its place; thoſe therefore in abſolute ſpace, and on the moving ſurface, muſt neceſſarily be the ſame, and conſequently the two tracks, *viz.* that on the moving ſurface, and that on the fixed one or about Z in abſolute ſpace, muſt in all caſes neceſſarily touch and roll. The fluxion of the track of O being $\sqrt{\dot\beta^2+\dot\gamma^2+\dot\delta^2} = \frac{\dot s}{s^3}\sqrt{\frac{\overline{\mathfrak{A}^2-BCe^2}^2}{\beta^2}+\frac{\overline{\mathfrak{C}^2-ABe^2}^2}{\delta^2}+\frac{A^2C^2e^4}{\gamma^2}}$, and the velocity along it = $\frac{\beta\gamma\delta}{ABC}\sqrt{\left(\frac{\overline{\mathfrak{A}^2-BCe^2}^2}{s^2\beta^2}+\frac{\overline{\mathfrak{C}^2-ABe^2}^2}{s^2\delta^2}+\frac{A^2C^2e^4}{s^2\gamma^2}\right)}$ becauſe $\dot\gamma^2 = \frac{A^2C^2e^4\dot s^2}{s^6\gamma^2}$, $\dot\delta^2 = \frac{\overline{\mathfrak{C}^2-ABe^2}^2\times\dot s^2}{s^6\delta^2}$, $\dot\beta^2 = \frac{\overline{\mathfrak{A}^2-BCe^2}^2\times\dot s^2}{s^6\beta^2}$, $\gamma^2 = \frac{AC}{s^2} \times \overline{e^2-s^2}$, $\delta^2 = \frac{\mathfrak{C}^2-AB\times\overline{e^2-s^2}}{s^2}$, $\beta^2 = \frac{\mathfrak{A}^2-BC\times\overline{e^2-s^2}}{s^2}$; hence $\frac{(\mathfrak{A}^2-BC\times\overline{e^2+s^2})\times(\mathfrak{A}^2-BC\times\overline{e^2-s^2})}{s^2\beta^2} = \frac{\overline{\mathfrak{A}^2-BCe^2}^2-B^2C^2s^4}{s^2\beta^2} = \mathfrak{A}^2 - BC \times$

$\times\sqrt{e^2+\varepsilon^2}$ in like manner $\frac{\sqrt{(\mathfrak{C}^2-ABe^2)^2-A^2B^2\varepsilon^4}}{\varepsilon^2\delta^2}=\mathfrak{C}^2-AB\times\sqrt{e^2+\varepsilon^2}$, and $\frac{A^2C^2\times\sqrt{e^4-\varepsilon^4}}{\varepsilon^2\gamma^2}=AC\times\sqrt{e^2+\varepsilon^2}$, hence the velocity $\frac{\sqrt{\dot\beta^2+\dot\gamma^2+\dot\delta^2}}{i}=\frac{\beta\gamma\delta}{ABC}\sqrt{\left(\frac{B^2C^2\varepsilon^2}{\beta^2}+\frac{A^2B^2\varepsilon^2}{\delta^2}+\frac{A^2C^2\varepsilon^2}{\gamma^2}+\mathfrak{A}^2-BCe^2-BC\varepsilon^2+\mathfrak{C}^2-AB\times\sqrt{e^2+\varepsilon^2}\right.}$ $\left.+AC\times\sqrt{e^2+\varepsilon^2}\right)=\frac{\beta\gamma\delta}{ABC}\sqrt{\left(\frac{B^2C^2\varepsilon^2}{\beta^2}+\frac{A^2B^2\varepsilon^2}{\delta^2}+\frac{A^2C^2\varepsilon^2}{\gamma^2}-\varepsilon^2\right)}$ (becauſe $1+AC-BC-AB=0$, and $\mathfrak{A}^2+\mathfrak{C}^2=e^2$) which may be farther reduced to $\frac{ey}{\varepsilon^2}\sqrt{\left(\frac{\mathfrak{C}^2}{A^2BC}+\frac{\mathfrak{A}^2}{ABC^2}-\frac{e^2}{AC}-\frac{\overline{A+C}\times\mathfrak{A}^2\mathfrak{C}^2}{A^2BC^2e^2}+\frac{\mathfrak{A}^2\mathfrak{C}^2}{B^2y^2}\right)}=$ the velo- with which the momentary pole O ſhifts its place along its proper track; but it ſhifts its place in a direction perpendicular to the great circle ZO at O with a velocity whoſe ſquare is equal to the ſquare of that laſt found *minus* the ſquare of $\frac{\dot{ZO}}{i}$ which is the velocity along $ZO=\frac{\dot\varepsilon}{i\varepsilon^2\times\text{ſ. ZO}}\times\varepsilon\times\text{cof. ZO}=\frac{\varepsilon\beta\gamma\delta}{ABC\times\text{tang. ZO}}$, hence then the velocity perpendicular to ZO at O $=\frac{\varepsilon\beta\gamma\delta}{ABC}\sqrt{\left(\frac{B^2C^2}{\beta^2}+\frac{A^2B^2}{\delta^2}+\frac{A^2C^2}{\gamma^2}-1-\frac{1}{\text{tang. ZO}^2}\right)}$ this drawn into i gives $\frac{\dot\varepsilon}{\varepsilon}\sqrt{\left(\frac{B^2C^2}{\beta^2}+\frac{A^2B^2}{\delta^2}+\frac{A^2C^2}{\gamma^2}-\frac{1}{\text{ſ. ZO}^2}\right)}=$ the elementary ſpace perpendicular to ZO; hence the angular velocity with which O ſhifts its place about Z in abſolute ſpace $=\frac{\varepsilon\beta\gamma\delta}{ABC\times\text{ſ. ZO}}\sqrt{\left(\frac{B^2C^2}{\beta^2}+\right.}$ $\left.\frac{A^2B^2}{\delta^2}+\frac{A^2C^2}{\gamma^2}-\frac{1}{\text{ſ. ZO}^2}\right)$, and the elementary ſpace divided by ſ. ZO gives the meaſure of the elementary angle, and the track of O in abſolute ſpace may hence, *conceſſis quadraturis*, be conſtructed by points. But this is unneceſſary after the path of one of the angles C of the octant has been found; ſince the track of O is thence given by the projection of points *ad libitum* of the now known triangle ZOC.

Hence then we collect, that the point Z is ſuch that the angular velocities at the points q, r, s, Y, in directions perpendicular to the great circles drawn through Z and the poles A, B, C, and O, meaſured at 90° diſtance from Z, are all conſtant quantities in all poſſible caſes, notwithſtanding the irregularity of the body's motion, which is a property very remarkable.

4. If $\frac{1}{A}$ here be $=0$, $=\frac{b^2-c^2}{a^2}$, and $b^2=c^2$, or the two leſs *momenta* of *inertia* are equal, which is the caſe of a ſquare priſm, cylinder, ſpheroid, or other ſolid of revolution; then $\varepsilon^2=e^2$ conſtant, $B=C$, $\delta^2=\frac{\mathfrak{C}^2}{e^2}-\gamma^2$, $\beta^2=n^2=\frac{\mathfrak{A}^2}{e^2}$ conſtant, $e^2\beta^2=x^2=\mathfrak{A}^2$, $e^2\delta^2=z^2=\mathfrak{C}^2-y^2$, $\dot{t}=-\frac{B\dot{y}}{\mathfrak{C}\sqrt{\mathfrak{C}^2-y^2}}=\frac{B\dot{z}}{\mathfrak{A}\sqrt{\mathfrak{C}^2-z^2}}=\frac{B\dot{\delta}}{\mathfrak{A}\sqrt{\frac{\mathfrak{C}^2}{e^2}-\delta^2}}=\frac{Be}{\mathfrak{A}\mathfrak{C}}\times\frac{\mathfrak{C}\dot{\delta}}{e\sqrt{\frac{\mathfrak{C}^2}{e^2}-\delta^2}}$, &c. as in the particular caſe conſidered in the 4th and 5th propoſitions, the A there being $=B$ here. And hence the velocity above of O in its track $=\frac{\mathfrak{A}\mathfrak{C}}{Be}=\frac{eb\beta}{B}$ as there found. Coſ. OZ = a conſtant quantity $=\frac{a^2\mathfrak{A}^2+c^2\mathfrak{C}^2}{e\sqrt{a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}}=\frac{a^2\beta^2+c^2b^2}{\sqrt{a^4\beta^2+c^4b^2}}$, and ſ. $OZ=\frac{b^2\mathfrak{A}\mathfrak{C}}{Be\sqrt{a^4\mathfrak{A}^2+c^4\mathfrak{C}^2}}=\frac{b^2\beta b}{B\sqrt{a^4\beta^2+c^4b^2}}=\frac{\overline{a^2-c^2}\times\beta b}{\sqrt{a^4\beta^2+c^4b^2}}=\frac{\beta b}{\sqrt{B^2+2B\beta^2+\beta^2}}$, as there found, &c. And therefore, when b is very ſmall, this is much ſmaller, being then nearly $=\frac{b}{B+1}$, which in the caſe of the *earth* is nearly $=\frac{b}{232}$, and therefore inſenſible. For on the hypotheſis that $b=9''$, this quantity, or half the diurnal nutation will be leſs than the $\frac{1}{25}$th part of a ſecond, and the whole *diurnal nutation* leſs than the $5'''$. Indeed the $\frac{1}{13}$th part

part of a ſecond muſt be very near the true quantity; for, though the earth's figure may not be preciſely that of a ſpheroid, it cannot differ from it ſo much as to make any ſenſible alteration in this, eſpecially now it appears from the foregoing general ſolution, that the angular velocity about the axis whoſe pole is Z is always uniform and conſtant, let the figure of the revolving body be what it will. Neither can the progreſſive or annual motion cauſe any alteration, becauſe it cannot at all affect the rotatory or diurnal one.

5. The remarkable property mentioned at the end of the 3d of theſe general ſcholia, may be more particularly expreſſed thus: as the ſ. $Zq = mb^2y : 1 :: y : \frac{1}{mb^2} = \frac{\sqrt{a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2}}{b^2}$ = the angular velocity at q about the axis whoſe pole is Z; in like manner, the angular velocity at r (fig. 4.) about the ſame axis = $\frac{\sqrt{a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2}}{c^2}$, that at $s = \frac{\sqrt{a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2}}{a^2}$, and that at $Y = s \text{ cof. } OZ = \frac{a^2\mathfrak{A}^2 + c^2\mathfrak{C}^2}{\sqrt{a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2}} = V = \sqrt{e^2 - \frac{\overline{a^2 - c^2}^2 \times \mathfrak{A}^2\mathfrak{C}^2}{a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2}}$, which, being the velocity of the moving ſpherical ſurface at every point of the great circle whoſe node is Y, and every point of that great circle being at the diſtance of 90° from Z, *the angular velocity of the body round the axis at reſt in abſolute ſpace whoſe pole is* Z *will be always equable, uniform, and conſtant, notwithſtanding the other oſcillating, vacillating motions of the body*: e being the greateſt angular velocity about the momentary axis.

This motion, then, is of the moſt ſimple and evident kind, and, together with that of the track under Z above determined, limits the whole compound motion under conſideration, all the others being only neceſſary conſequences of theſe;

ſo

ſo that after all the pains beſtowed upon the problem, the reſult is as ſimple as could be wiſhed for; and the motion, though not quite ſo regular, is as eaſy to be conceived as that in the particular caſe of the ſolids of revolution. For the ſpherical ſurface, concentric with the body, moves with an uniform and conſtant angular velocity V about an axis IZ at reſt in abſolute ſpace, whilſt the track WV′W′V upon that ſurface always paſſes Z, the pole of that axis, with a velocity $= \sqrt{\frac{b^4\mathfrak{A}^2\mathfrak{C}^2}{B^2 \times a^4\mathfrak{A}^2 + c^4\mathfrak{C}^2} - \frac{y^2}{AC}}$, which, though not conſtant, recovers its firſt value again and again in equal times, as the body revolves for ever.

6. I ſhall only juſt add, that if P, Q, and R, be any three external motive forces ſuppoſed to act upon the body in the directions of the three great circles BC, CA, and AB, then muſt $\frac{P}{Ma^2} = \frac{\dot{x}}{t} - \frac{b^2 - c^2}{a^2} \times yz$, $\frac{Q}{Mb^2} = \frac{\dot{y}}{t} - \frac{c^2 - a^2}{b^2} \times zx$, and $\frac{R}{Mc^2} = \frac{\dot{z}}{t} - \frac{a^2 - b^2}{c^2} \times xy$ expreſs the values of the external accelerating forces that act upon the body to alter its velocity about the three permanent axes of rotation. And when the relations of thoſe external forces to the internal perturbating ones are given, a ſolution will hence be obtained to the more general problem, for determining the motion of the body, when, beſides the perturbation ariſing from the centrifugal force of its own particles, it is alſo acted upon by any external diſturbing forces whatever. And, if P, Q, and R, be equal to, but in contrary directions to $Myz \times \overline{b^2 - c^2}$, $Mzx \times \overline{c^2 - a^2}$, and $Mxy \times \overline{a^2 - b^2}$, the perturbations vaniſh, and then about whatever axis the body is firſt impelled, it muſt continue to revolve uniformly round it for ever.

Fig. 1.

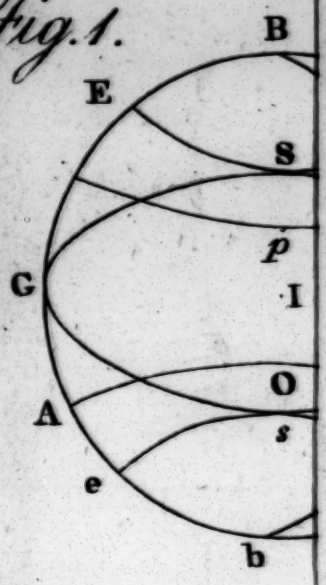


Fig. 5.

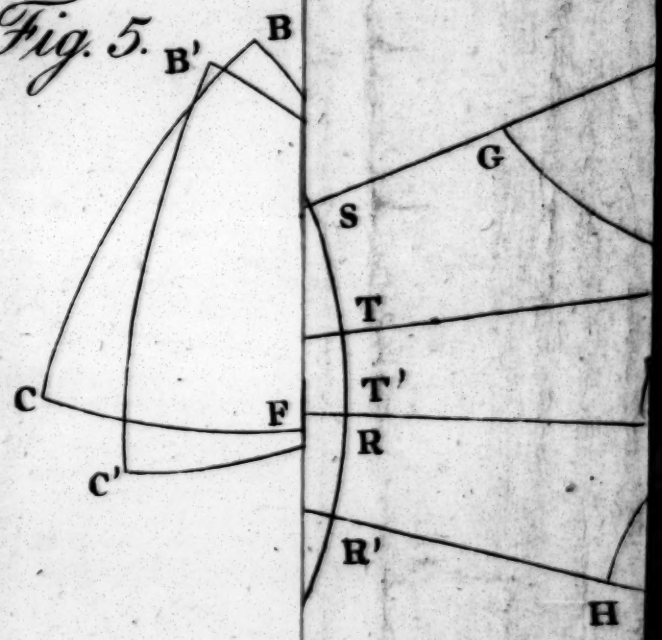


Fig. 8.

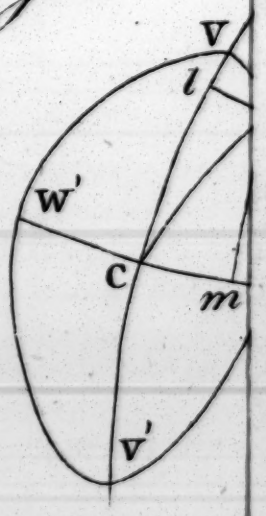


Fig. 1.

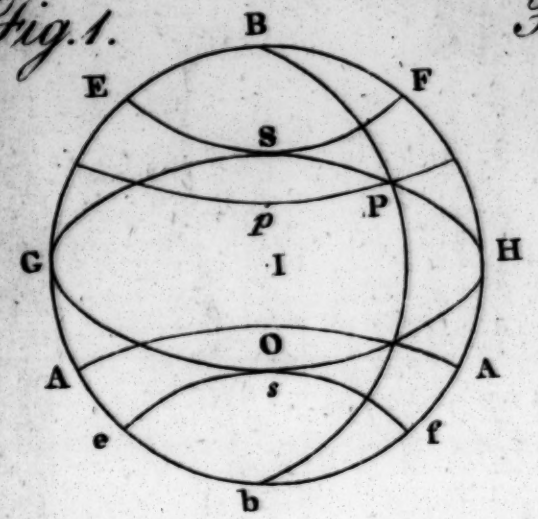


Fig. 2.

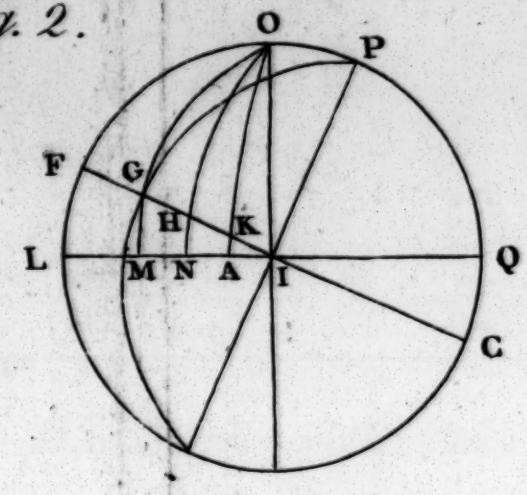


Fig. 3.

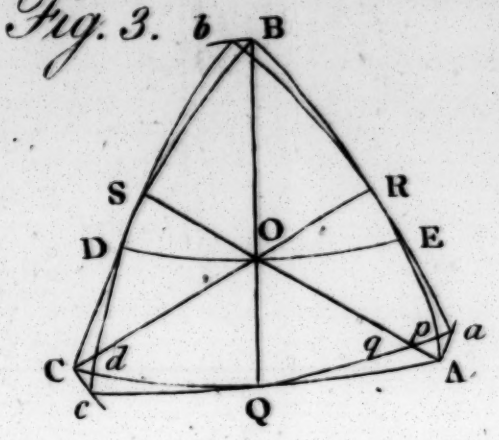
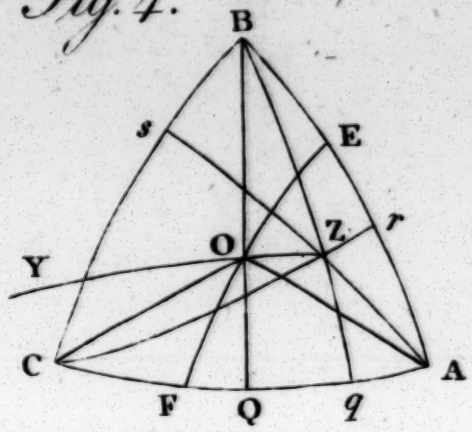


Fig. 4.



V

Z

Fig. 5.

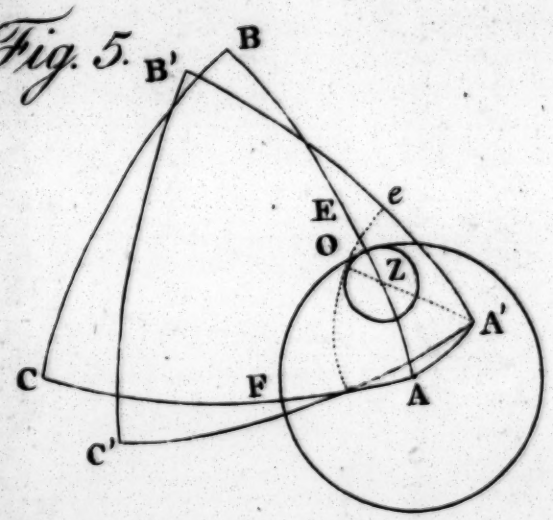


Fig. 6.

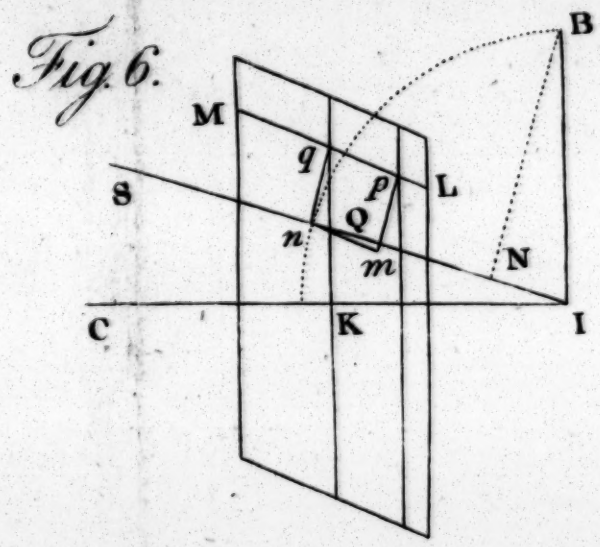


Fig. 7.

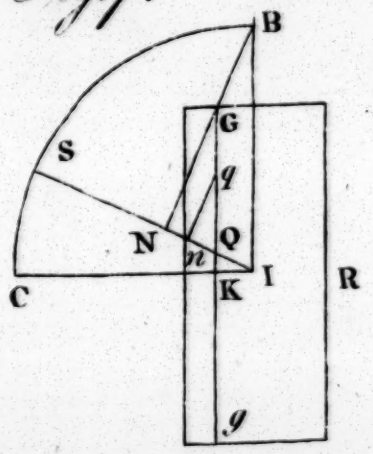


Fig. 8.

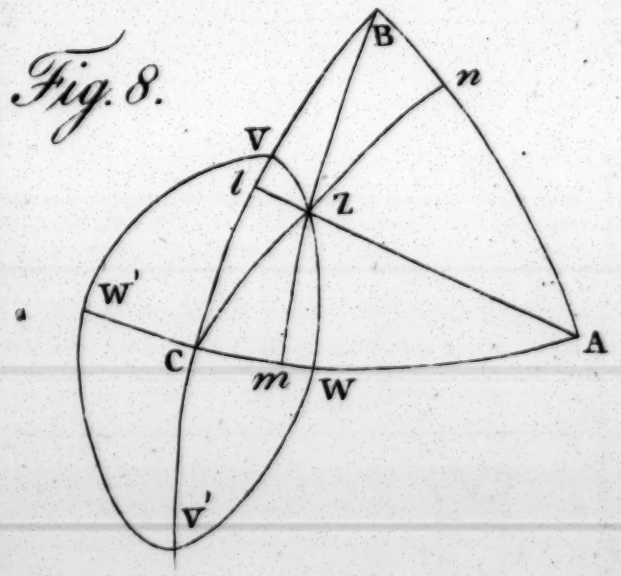


Fig. 9.

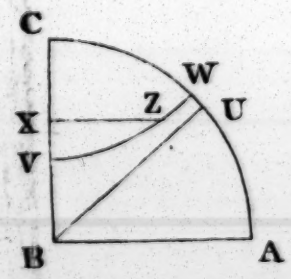


Fig. 10.

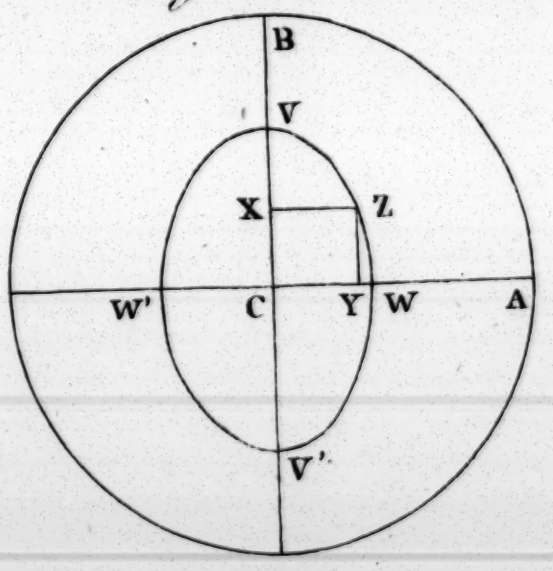
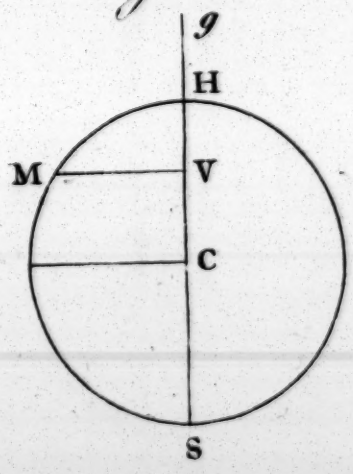


Fig. 11.



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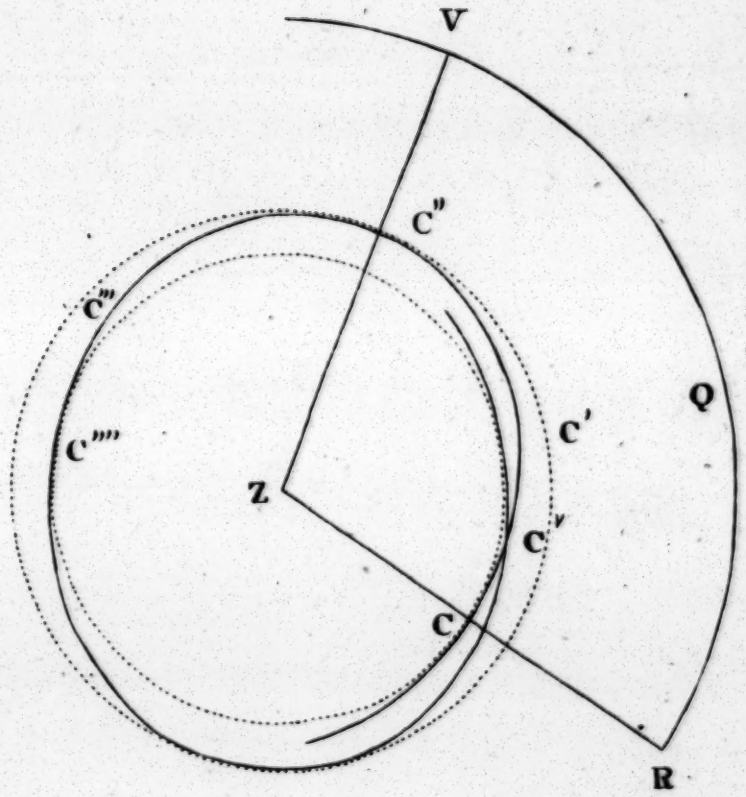
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Fig. 12.



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Note referred to in page 519.

(C) Without any regard to the parallelopipedon, let the form of the body be what it will, if the *momenta* of *inertia* round the three permanent axes be repreſented by M*aa*, M*bb*, and M*cc*, the relative motive forces round thoſe axes will always be expreſſed by Ma^2x^2, Mb^2y^2, and Mc^2z^2, acting at the diſtance of radius therefrom. And then, in fig. 7., the centrifugal motive force acting along BI, being $=Mb^2y^2$, that acting along BN at N will, by the laws of central force, be $=Mb^2y^2 \times \frac{BI}{BN}$; and therefore the equivalent one, acting at S perpendicular to SI, will be $=Mb^2y^2 \times \frac{NI}{BN} = Mb^2y^2 \times \frac{x}{y} = Mb^2yx$ urging the point S towards B. In like manner it is found, that the centrifugal motive force Ma^2x^2 acting along CI produces one at S perpendicular to SI $=Ma^2xy$ urging it towards C; and the difference of theſe $=Ma^2xy - Mb^2xy$ muſt be the perturbating motive force at S, along the great circle BSC, as found by the other methods. And in the very ſame manner may thoſe in the other great circles bounding the octant be found.



XXV. *On the Chronology of the Hindoos.*

By William Marſden, *Eſq. F. R. S. and A. S.*

Read June 24, 1790.

IT is my deſign to treat, in the following Paper, of the CHRONOLOGICAL ERAS in uſe among the people of Aſia, called *Hindoos*, who profeſs in general the religion of *Brahma*, and are conſidered as the indigenous inhabitants of India. The elaborate reſearches into their recondite writings, made by thoſe gentlemen who have applied themſelves to the ſtudy of *Sanſkreet* literature, afford us ground to expect conſiderable diſcoveries in regard to the ſcientific attainments of this ancient and celebrated people. Their aſtronomy in particular has attracted the attention of the learned in Europe, who ſee in it the internal evidence of very remote antiquity, and are diſpoſed to allow its pretenſions to originality; many ſtrong and unequivocal traces indicating that the Greek ſchool at leaſt derived more than the elements of their knowledge, by no very circuitous channel, from this oriental ſource.

Unfortunately for the gratification of rational curioſity, hiſtory ſeems to have been, of all branches of ſtudy, that which the Hindoos cultivated with the leaſt care, and we regret to find the periods marked by the revolutions of the heavenly bodies, of which other nations have availed themſelves to aſcertain and record the important events in human

affairs, by them unprofitably applied to the dreams of their mythology. The unremitted labour of ages has been devoted to perfecting the calculation of the lunar motions, in which their correctneſs is ſurpaſſed only by the European improvements of very modern times; but, by a ſtrange perverſion, the accuracy thence acquired in their prediction of eclipſes, appears to have no other object than that of adminiſtering to an idle ſuperſtition, which it ought to deſtroy.

Although the *fabulous* exceedingly prevails in all the ancient documents hitherto introduced to our knowledge, and in none more conſpicuouſly than thoſe which contain the genealogies and reigns of their early kings (*a*)*, yet we are not haſtily to conclude, that this people are deſtitute of records of true hiſtory. The portion of their literary ſtores which we have had opportunity to examine is comparatively ſmall. Perſeverance may diſcover annals, more or leſs ancient, whoſe preſent obſcurity is perhaps occaſioned by that very circumſtance which conſtitutes their real value — the want of the *miraculous*. Some authentic monuments have already been elucidated by the learned ſkill of a gentleman, now a Member of the Royal Society. Facts will accumulate by degrees, and acquire authority by mutually bearing on each other; and the Hindoos, like many other nations of the world, may hereafter be indebted to ſtrangers, more enlightened by philoſophy than themſelves, for a rational hiſtory of their own country.

In different parts of India, and even in one and the ſame part, we obſerve various chronological eras referred to, as well in their aſtronomical treatiſes, as in their political and private writings. Theſe being productive of confuſion, if not clearly underſtood and diſcriminated, I ſhall endeavour to exhibit ſuch a comparative ſtatement of their reſpective commencements

* See the Notes at the end of the Paper.

and coincidences as may tend to remove this impediment to the progreſs of hiſtorical knowledge. I am not aware that any attempt has hitherto been made in Europe, to bring them into one point of view; and as an opportunity is hereby given of correcting ſome errors that we have been led into by the ſanction of reſpectable names, I am induced to think, that what I have undertaken will not be conſidered as ſuperfluous.

My preſent purpoſe does not lead me to attempt a diſcuſſion of what may be termed the *factitious* periods of Hindoo computation, or to explain the nature and duration of the four ages, or *Yoogs*, which this ſpeculative people, in the wanton exerciſe of numerical power, have portioned out from the boundleſs region of time. The three former of theſe diviſions, even though the progreſſive numbers aſſigned to them ſhould be admitted as the reſult of aſtronomical combination, can be preſumed to have but little reference to practical chronology, which ſeems to trace its origin no higher than the commencement of the fourth, or preſent age, denominated the *Kalee Yoog*. This conſtitutes the principal era to which I would draw attention, and comprehends within it theſe that follow; the era of *Bikramajit*, the era of *Salabân*, the Bengal era (not ſtrictly Hindoo), and the cycle of ſixty years.

Before I proceed to a compariſon of their ſeveral dates, it will be proper to define the nature of the year and its conſtituent parts, according to which theſe eras are computed by the *Brahmans*, who are the depoſitaries of ſcience as well as of religion. Their aſtronomical year is the meaſure of that portion of time which is employed in a revolution of the ſun, from the moment of his departure from a certain ſtar in their zodiac, as ſeen from the earth, till his return to the ſame. It is therefore ſolar and ſydereal, and contains, by their calculation,

365 d. 6 h. 12′ 30″; and as they ſuppoſe the annual movement of the ſtars in longitude, or the preceſſion of the equinoxes, to be 54 ſeconds of a degree, or 21′ 36″ of time (admitting, with them, the ſun to move at the rate of a degree each day), their tropical year will be 365 d. 5 h. 50′ 54″ (*b*): but as the ſun really moves over 54″ in 21′ 55″, its length is ſtrictly 365 d. 5 h. 50′ 35″, or 1′ 52″ greater than the tropical year as determined by MAYER at 365 d. 5 h. 48′ 43″. The true preceſſion being 50″,3, which ſpace the ſun deſcribes in 20′ 25″, the true ſydereal year is 365 d. 6 h. 9′ 8″, and conſequently the Hindoo year exceeds it by 3′ 22″, or one day in 430 years. If the opinion of aſtronomers is well founded, that a ſenſible diminution in the length of the year, as well as in the angle of obliquity of the ecliptic, has gradually taken place in the lapſe of many ages, it will follow, that this error may not have exiſted, or been ſo great, at the period of adjuſting the Hindoo tables: and when we conſider that there appears no ground to believe their apparatus for obſerving was ever much ſuperior to what it is diſcovered amongſt the Brahmans of this day (*c*), we are led to wonder at the preciſion attained to in this determination, and which, in the calculation of the moon's apogee, is ſtill more remarkable. The defect of art can have been compenſated only by the remote antiquity in which the ſeries of their obſervations originated, affording an opportunity of correcting the inaccuracy of particular meaſurements by a mean of large numbers and diſtant intervals.

They divide the zodiac into twenty-eight lunar and into twelve ſolar conſtellations or ſigns (*d*), and their aſtronomical year commences with the ſun's arriving at the firſt point of their conſtellation of *Aries*. This diviſion of the zodiac, ſo far as the accuracy of their obſervations allows, is connected

with the actual phænomena of the heavens, and advances with the apparent motion of the ſtars, from eaſt to weſt, leaving gradually behind it the equinoctial points, and is not, like our zodiac, an abſtract diviſion of ſpace, attached to thoſe points, and independent of the ſtarry ſyſtem. Calculating on their principles, the difference of the two zodiacs, or the accumulated amount of the annual preceſſion, ſince the coincidence ſuppoſed to be in the year of the Chriſtian era 499, is in the the preſent year 19° 21′ 54″.

The length of their months is determined by the time which the ſun employs in paſſing each ſign, and they are accordingly longer in the *apogee*, and ſhorter in the *perigee*; that which correſponds with the higher *apſis* being 31 d. 14 h. 39′, and that with the lower 29 d. 8 h. 21′ only. It does not appear, that the Hindoos are accuſtomed to enumerate, for civil purpoſes, the days of the ſolar month, but to date from the age of the moon that happens to fall within that month, or frequently from the ſimple phaſe of the moon (*e*).

Their feſtivals and faſts, like thoſe of the Jews and Chriſtians, being regulated, for the moſt part, by the lunar revolutions, they employ on this account, excluſively of the ſolar, aſtronomical reckoning, a lunar year (*f*). This they make to conſiſt ordinarily of twelve months, and each ſemi-lunation is diſtinguiſhed into fifteen equal portions, or lunar days, which are ſomewhat ſhorter than the natural day. In order to preſerve its general correſpondence with the ſolar year, "they reckon "twice that lunation during which the ſun does not enter upon "any new ſign" (*g*), or, in other words, which falls completely within a ſolar month; and the obvious reaſon for this mode of intercalating is, that as the lunar months take their denomination from the ſolar month in which the *change* happens, if two new

moons

moons fall within the ſame month, they naturally take the ſame name, and no irregularity is obſervable. This opportunity of encreaſing the number of lunar months, without embarraſſing the reckoning, preſents itſelf, on a medium of a few years, juſt as often as is requiſite to effect the compenſation. It cannot happen in all the ſolar months indifferently, their lengths being unequal, and ſome of them ſhorter than the ſynodical lunar month.

The commencement of the ſolar day is uſually eſtimated from ſunriſe, and the ſpace between that and the ſunriſe of the following day is divided into *ſixty* parts, the length of which muſt vary with the ſun's unequal courſe through the ecliptic; but for the purpoſes of calculation it is ſuppoſed to be aſcertained at the ſolſtices, and is equal to twenty-four of our minutes. The ſubdiviſions, in like manner, follow the ſexageſimal ſcale. There is alſo a mechanical diviſion of the day and night into *eight* parts, of which four are allowed to the interval from ſunriſe to ſunſet, and four to that from ſunſet to ſunriſe. The proportion of length of theſe parts reſpectively depends therefore upon the ſeaſon of the year and the latitude of the place, and the diviſion is conſequently inapplicable to general aſtronomy.

The days of the week are denominated from the ſeven planets, and their arrangement is the ſame with that adopted in the weſtern parts of the world, proceeding from the ſun and moon to Mars, Mercury, Jupiter, Venus, and Saturn (*h*). Friday, or the day of Venus, appears as the firſt of the week in their calculations, and probably becauſe the *Kalee Yoog* began on that day; but, in common, the week is conſidered by the Hindoos as beginning with Sunday.

Having

Having thus briefly touched upon ſuch points of their aſtronomy as are immediately connected with the meaſurement of time, I proceed to the compariſon of their *eras*, which I ſhall firſt bring into one general view, and afterwards conſider ſeparately.

Table exhibiting the correſpondence of the ſeveral *Hindoo eras* with each other, and with the Julian period and Chriſtian era.

	Julian period.	*Kalee Yoog.*	Era of *Bikramajit.*	Chriſtian era.	Era of *Salabân*	Bengal era.	Cycle of 60 years.
Kalee Yoog, or grand era	1612	0					13
Era of *Bikramajit* .	4657	3045	0				58
Chriſtian era . .	4713	3101	56	0			54
Era of *Salabân* .	4791	3179	134	78	0		11
Preſent cycle of 60 years	6460	4848	1803	1747	1669	1154	1
Year 1790 of J. C. (from April) .	6503	4891	1846	1790	1712	1197	44

For better underſtanding the above table, it is neceſſary to remark, that when the Hindoos quote the year of an era, they do it by the number of the elapſed or complete year; whereas, the common European mode is to date by the number of the current or incomplete year; therefore what we ſhould term the firſt year of an era, is with them the year *zero*, and their year *one* that which follows; excepting in the cycle of ſixty, of which the year *one* immediately ſucceeds the laſt complete year of the former cycle. The difference in years between any two eras is expreſſed by the number appearing at the interſection of the horizontal and perpendicular lines, to which the names

names of ſuch eras are prefixed; or it may be found by ſubtracting the leſſer number from the greater, as they ſtand in any of the horizontal lines, under their reſpective names at the top of the perpendicular columns; thus, the years intervening between the era of *Salabân*, and that of the *Kalee Yoog*, are denoted by the number 3179, at the interſection of the two lines, or equally by deducting 1712 from 4891, in the loweſt horizontal line.

In a compariſon of the dates of the earlier Hindoo eras with the Chriſtian era, there occurs a difficulty which it is proper to conſider apart. This ariſes from an ambiguity in our manner of reckoning the years before Chriſt. It is moſt uſual to paſs immediately from the year *one after* to the year *one before* Chriſt, making the interval of time only one year; but ſome of the beſt chronologiſts paſs from the year *one after* to the year *zero*, and from thence to the year *one before*; by which means the interval between any number of years before and after Chriſt is equal to the ſum of thoſe numbers; and as this method is uſed in almoſt all aſtronomical tables, it may, without impropriety, be called the *aſtronomical*, and the other the *common* method. The following rules will facilitate the reduction of the ſeveral Hindoo years to thoſe of the Chriſtian era, and to the years before it.

To find the year of Chriſt corresponding with a given date of the

- *Kalee Yoog*,
- E. of *Bikramajit*,
- E. of *Salabân*,
- Bengal era,
- Preſent cycle, of 60 years,
- Enſuing cycle,

when the date is poſterior to our era,

- ſubtr. 3101 from it.
- ſubtr. 56 from it.
- add 78 to it.
- add 593 to it.
- add 1746 to it.
- add 1806 to it.

But

But when the date is prior to our era, ſubtract the year of { Kalee Yoog from { 3102 for *common* method. 3101 for *aſtronomical* method. / Bikramajit from { 57 for *common* method. 56 for *aſtronomical* method.

As the Hindoo year begins in our month of April (which will be particularly explained), we muſt obſerve, in reducing any Hindoo date to the Chriſtian era, that when it happens between the commencement of our year and theirs, the number of their year muſt be increaſed by one, and the ſubtraction or addition then made as above directed.

The *Kalee Yoog*, or principal chronological era, began in the year juſt ſpecified, when the ſun's mean place was in the firſt point of the conſtellation *Aries* of the Hindoo zodiac; which happened on the 18th of February, at ſunriſe (*i*), under their firſt meridian, called the meridian of *Lanka*. At that period, it is ſaid to be aſſerted by their aſtronomers, that the ſun, moon, and all the planets, were in conjunction, according to their mean places. The reality of this fact, but with conſiderable modification, has received a reſpectable ſanction from the writings of an ingenious and celebrated Member of the French Academy of Sciences, who concludes, that the actual obſervation of this rare phænomenon, by the Hindoos of that day, was the occaſion of its eſtabliſhment as an aſtronomical epoch (*k*). Although M. BAILLY has ſupported this opinion with his uſual powers of reaſoning, and although abundant circumſtances tend to prove their early ſkill in this ſcience, and ſome parts of the mathematics connected with it (*l*), yet we are conſtrained to queſtion the verity or poſſibility of the obſervation, and to conclude rather that the ſuppoſed conjunction was, at a later period, ſought for as an

epoch, and calculated retroſpectively. That it was widely miſcalculated too, is ſufficiently evident from the computation which M. BAILLY himſelf has given of the longitudes of the planets at that time, when there was a difference of no leſs than 73° between the places of Mercury and Venus. But fifteen days after, when the ſun and moon were in oppoſition, and the planets far enough from the ſun to be viſible, he computes that all, excepting Venus, were comprehended within a ſpace of 17°; and on this he grounds his ſuppoſition of an actual obſervation.

Calculating from the rules laid down by the Brahmans (as given by M. LE GÉNTIL), it appears, that 4891 mean years of this era expired on Monday, 12th April, 1790, at 4½ h. and that the true place of the ſun came to the firſt of Aries, and conſequently that the 4892d year (or 4891 complete, as the Hindoos expreſs it) actually began on the 10th, at 1 h., or on Sunday, 11th April, in their civil way of reckoning (*m*). Thus we ſee that, during this long period of time, the Hindoo account has loſt upon the Julian 42 days, allowing for the change that took place in our ſtyle. The year of the former exceeding the latter by 12′ 30″, falls continually later and later on our old ſtyle year, at the rate of a day in about 115 years; and from this the commencement of any future year may be readily computed. The annual irregularity obſervable, which is independent of this almoſt imperceptible change, ariſes only from our mode of intercalating a day at the end of every fourth year, to compenſate the fractions that have accumulated during that time. The Hindoo aſtronomical year, admitting of no intercalation, cannot preſerve an annual correſpondence, but began at nearly the ſame time, with reſpect to our year, in 1786 as in 1790; in 1775

and 1787, six hours later; in 1781 and 1785, six hours sooner; and in 1784 and 1788 (being leap years) eighteen hours sooner than in 1787. The civil year begins at the sunrise immediately preceding the calculated commencement of the astronomical, when that happens during the day; but if in the night time, it begins at the sunrise following, and will consist of 366 days so often as the excess of the astronomical year above 365 amounts to a whole day.

The next era that presents itself, and the first that has pretensions to be considered as historical, is that of *Bikramajit* (*n*). This prince, whose paternal kingdom was *Malwa*, and his capital *Ougein*, waged war against *Saka* king of *Dehli*, probably his lord paramount, and having overcome and slain him, ascended in his stead the principal throne of India. Authorities differ widely as to the length of his reign (*o*); but he likewise is said to have fallen in battle, fighting with a king who invaded him from the southern provinces. He appears, from this account, in no other light than that of an unfortunate usurper, yet his fame in the memory of the Hindoos has eclipsed that of his predecessors, his adventures are a favourite subject of romance, and an era has been distinguished by his name. It is doubted, whether we ought to consider his accession, or his death, as constituting the proper epoch; but with regard to the time from which the reckoning dates there is no uncertainty, and the nature of the event is not essentially connected with our present object. It is placed in the year 56 before Christ, or in the 4657th of the Julian period, and corresponds with the 3045th year of the grand era (*p*).

I had occasion to remark, when treating of the *Hejerà*, that this Mahometan era was computed, not from the day of the Prophet's flight, as generally supposed, but from the ordinary

commencement of that year in which the flight happened (*q*); and thus we find, on comparing the Hindoo eras, that though ſome of them are profeſſed to be counted from the deaths of their kings or other hiſtorical events, they yet all begin from the ſame point of the ſun's annual courſe through the zodiac. The numerical reckoning of the years can well be conceived more liable to arbitrary change, as being leſs intereſting to the bulk of the people, than the obſervance of a particular day, whoſe periodical return is every where marked with popular ceremonies and ſuperſtitions.

The era to which *Salabân* has given name dates from the 78th year of Chriſt, or 4791ſt of the Julian period, commencing with the 3179th year of the grand era. As this is no leſs than 134 years later than that of *Bikramajit*, it ſeems a bold anachroniſm to make them cotemporaries, or to ſuppoſe, what is commonly aſſerted, that the one prince was the conqueror of the other (*r*). Fortunately for my preſent inveſtigation, it is hiſtory rather than chronology which ſuffers from this want of accuracy or diſcrimination in the annaliſts of India, or their Perſian tranſlators. *Salabân*, who is ſaid to have reigned many years over the ancient kingdom of *Narſinga*, in the northern part of the peninſula, is deſcribed as a liberal encourager of the ſciences. There is reaſon to think, that aſtronomy experienced a reformation and conſiderable improvements under his auſpices; and the profeſſors appear to have attached the celebrity of an era to his death, in reſpect for his talents and gratitude for his protection.

As the era of *Bikramajit* prevails chiefly in the higher or northern provinces of India, ſo does that of *Salabân* in the ſouthern, but more excluſively. In their current tranſactions, however, the inhabitants of the peninſula employ a mode of

computation of a different nature, which, though not unknown in other parts of the world, is confined to theſe people amongſt the Hindoos. This is a cycle, or revolving period, of ſixty ſolar years, which has no further correſpondence with the eras above mentioned than that of their years reſpectively commencing on the ſame day. Thoſe that conſtitute the cycle, inſtead of being numerically counted, are diſtinguiſhed from each other by appropriate names, which, in their epiſtles, bills, and the like, are inſerted as dates, with the months, and perhaps the age of the moon annexed; but, in their writings of importance and record, the year of *Salabân* (often called the *Saka* year) is ſuper-added; and this is the more eſſential, as I do not find it cuſtomary to number the cycles by any progreſſive reckoning (*s*). In their aſtronomical calculations, we obſerve, that they ſometimes compute the year of their era by multiplying the number of cycles elapſed, and adding the complement of the cycle in which it commenced, as well as the years of the current cycle; but from hence we are led to no ſatisfactory concluſion reſpecting the origin of this popular mode of eſtimating time (*t*). The preſumption is in favour of its being more ancient than their hiſtorical epochs. The preſent cycle, of which 43 complete years were expired in April 1790, began in the year 1747, with the year of *Salabân* 1669, and of the grand era 4848. M. LE GENTIL, to whom Europe is chiefly indebted for what is known of Hindoo aſtronomy, has fallen into an unaccountable error with regard to the years of this cycle, and their correſpondence with thoſe of the Kalee Yoog, as appears by the comparative table he has given of them, and other paſſages of his work. He ſeems to have taken it for granted, without due examination, that the year 3600 of the latter muſt have been produced by the multiplication of the

the cycle of ſixty into itſelf, and conſequently that the firſt year of this grand era muſt alſo have been the firſt of the cycle; but this is totally inconſiſtent with the fact; the Kalee Yoog began with the 13th year of the cycle of ſixty, and all the reaſoning founded on the ſelf-production and harmony of theſe periods muſt fall to the ground (*u*).

It now remains to take notice of a mode of reckoning peculiar to the province of *Bengal*, and thence denominated the *Bengal* era. The circumſtances of its inſtitution are involved in obſcurity, and I do not find that even a conjecture on the ſubject has yet been offered to the world. It is admitted, however, to have been impoſed by the Mahometan conquerors, and is therefore of no very remote antiquity. The moſt obvious conſideration that preſents itſelf, in examining the date of this era, is its proximity to the year of the Hejerà; the Bengal year 1196 complete ending on the 10th April, 1790, and the Hejerà year 1204 on the 10th September following. The difference has plainly ariſen from the inequality of the ſolar and lunar reckonings, and its accumulation ſince a certain period when they muſt of neceſſity have coincided; and it is no improbable ſuppoſition, that the time of ſuch coincidence was alſo that of introducing the mode of computation which has ſince prevailed. By aſcertaining the amount of this difference, and the number of years required to produce it, we may expect to arrive at a knowledge of the period in queſtion, or to approximate it at leaſt. As the Hindoos compute from the elapſed year, and the Mahometans by the current, the difference between the two dates ſhould be ſix years and ſeven months; but as this correction of one year may be preſumed equally applicable at the ſuppoſed time of the coincidence, and therefore unneceſſary in this inſtance, it will follow that the real

difference

difference ſhould be found by a ſimple ſubtraction of one date from the other, and conſequently be ſeven years and ſeven months. The annual exceſs of the Hindoo or ſydereal year above the Mahometan or lunar being 10 d. 21½ h., an interval of 254 years is required to produce this difference, or 220 years, to produce that of ſix years and ſeven months. The former number being deducted from 1790, carries us back to the year 1536, in the reign of the Mogul emperor *Humaioon*, and the latter to 1570, in that of *Akbar*. About one or other of theſe periods we ſhould ſeek for ſome record of the inſtitution, but the hiſtories we poſſeſs throw no light upon the ſubject. Several gentlemen, converſant in the affairs of Bengal, to whom I have referred it, confirm the general idea as above given, but diſagree as to the political circumſtances. By one, the regulation is aſcribed to *Sheer Shah*, who wreſted the empire from *Humaioon*, and governed it for ſome years with wiſdom and energy, when his death, and the diſtractions that enſued, reſtored it to the former poſſeſſor; and, by another authority, to *Akbar*, deſervedly named the Great, who was next in ſucceſſion.

The moſt obvious way of accounting for the peculiar mixture of Hindoo and Mahometan obſervances in this reckoning, appears to be, that the zeal of the monarch for eſtabliſhing the era of his prophet had effected only a partial or temporary innovation; and that his new ſubjects, who were conſtrained to adopt as an epoch that year of the Hejera in which the royal edict bore date, could not ultimately be forced to change their accuſtomed ſolar year for one that rapidly inverted the order of the ſeaſons. But this attempt muſt be referred to a period ſomewhat earlier than the reigns of theſe particular princes, which were diſtinguiſhed by a liberal policy; and the

gentlemen

gentlemen above alluded to attribute to them reſpectively, not the interdiction but the *reſtitution* of the ſolar reckoning; that of the Mahometans, impoſed on the province by their more bigotted predeceſſors, being found inconſiſtent with the periodical collection of the revenue, which depends on the harveſts (*v*). It is, in truth, extremely difficult to conceive, how a mode of computation, ſo much at variance with the rational concerns of civilized people, can poſſibly ſubſiſt in any ſtate of ſociety above that of the paſtoral and predatory tribes with whom it originated.

As it appears, that the people of *Siam*, in the farther India, have borrowed their knowledge of aſtronomy from the Hindoos, it will not be thought inconſiſtent with the ſubject I am treating, to add ſome account of the chronological eras in uſe amongſt them. Of theſe, one has been termed their civil, and another their aſtronomical era. The civil reckoning is by lunar years, conſiſting ordinarily of 12 months each, with an intercalation of 7 months in the period of 19 years, and commencing with the new moon that precedes the winter ſolſtice. This era is computed from the ſuppoſed time of the introduction of their religion by *Sommona-codom*, 544 years before Chriſt, or in the year of the Julian period 4109; and conſequently 2333 years of it were expired in the month of December, 1789; but by a cuſtom which, though not without its parallel, wants to be ſatisfactorily explained, they do not change the date, or count the ſucceeding year 2334, till it meets the aſtronomical reckoning in the month of April following.

The aſtronomical era is founded immediately upon the tables and modes of calculation adopted from the Hindoos. The French aſtronomer, Dom. Cassini, by an ingenious deduction from

from no very circumſtantial *data*, inferred that it muſt have had for its epoch a mean conjunction of the ſun and moon, which happened on 21ſt March, 638 of the Chriſtian era. This preceded by a few hours the commencement of the eſtabliſhed Hindoo year, to which it was evidently meant to be accommodated, though it is by him referred to the vernal equinox, which took place two days earlier (*w*). The length of the Siameſe ſolar year he found to be 365 d. 6 h. 12′ 36″ (*x*), and conſequently 1152 years of the era ſhould expire on the 11th April, 1790, when the ſun enters the Indian zodiac, being 560 years later than the era of *Salabân*. For want of corroborating facts, this determination of an epoch by M. CASSINI was conſidered as ſpeculative and uncertain; but I am accidentally in poſſeſſion of a date, which, though not preciſe, may ſerve generally to authenticate it. "In 1769, the king of *Pegu* (a country bordering on *Siam*, and formerly conquered by it) dates his letter to the French at Pondicherry, the 12th of the month *Kchong* 1132." This makes 1790 to correſpond with 1153 inſtead of 1152; but when we conſider the vague manner in which notices of this kind are given, a difference of one year can ſcarcely be urged as an objection.

The Siameſe were alſo accuſtomed to make uſe of a cycle of ſixty years, expreſſed by a repetition of twelve names of certain animals, which, I obſerve, are for the moſt part the ſame with thoſe employed, for the ſame purpoſe, by the Chineſe and Mogul Tartars, from whom we may conclude it has been borrowed (*y*); but the meager and unſatisfactory examples of its application, furniſhed by M. LOUBERE and P. TACHARD, do not afford us the means of determining at what time they began to reckon their cycle. It appears only that the

year 1687 was the tenth of the leſſer or conſtituent cycle of twelve years (z).

In the foregoing account of the Indian eras, I am ſenſible that I have omitted many circumſtances, the detail of which, if accurately given, might be uſeful and intereſting; but at this diſtance from the countries where thoſe eras are employed, the means of information are limited, and particularly with reſpect to the variations prevailing in the different provinces of what was formerly the Mogul empire. Unfurniſhed myſelf with local knowledge, my objects have been to compare and reconcile the information already given to the world with the experience of perſons who have reſided in Hindoſtan, and to correct and ſubſtantiate the whole by bringing it to the teſt of modern accuracy in the ſcience of aſtronomy. In fulfilling the laſt eſpecially I confeſs myſelf under obligations for eſſential aſſiſtance.

NOTES.

(*a*) See the French tranſlation of the *Bhagavat*, under the title of *Bagavadam* ou *Doctrine divine*, p. 187. et ſeq. Compare alſo the liſts of ancient kings of *Dehli*, in BERNOULLI, Deſcription de l'Inde, Vol. I. p. 153. (from P. TIEFFENTHALER), and Vol. II. p. xxxix. (by M. ANQ. DU PERRON); in the former of which a celebrated *Rajah* is made to reign 90 years, and in the latter no more than 3 years.

(*b*) M. LE GENTIL, Voyage dans les Mers de l'Inde, Vol. I. p. 231. According to the Hindoo Treatiſe of Aſtronomy, called the *Soorya Seedhanta* (of which I have ſeen ſome tranſlated extracts), the length of the ſydereal year is 365 d. 6 h. 12′ 36″ 33‴ 36⁗, or about 6″½ longer than that given by M. LE GENTIL.

(*c*) Phil. Tranſ. Vol. LXVII. p. 598. ſee an account, by Sir ROBERT BARKER, of the Obſervatory at *Benares*; and in BERNOULLI, Vol. I. p. 316. and 347. (from TIEFFENTHALER) of thoſe at *Jêpour* and *Ougein*, conſtructed by *Rajah Jêſing*, in the preſent century.

(*d*) The Hindoo names of the ſolar ſigns, and of the months with which they correſpond, as alſo of the lunar conſtellations or manſions, are as follows:

Solar Signs.	Months.	
1 *Mēſh*	*Bīſāk* or *Viſāk* *	April
2 *Breeſh* or *Vreeſh*	*Jeat* or *Jēſht*	May
3 *Meetoon*	*Aſār*	June
4 *Karkat*	*Sāwan* or *Srāvan*	July
5 *Seengh*	*Bhādoon*	Auguſt
6 *Kanyā*	*Āſan*	September
7 *Toolā*	*Kārteek*	October
8 *Vreeſcheek*	*Āghan* or *Agrahan*	November
9 *Dhanoo*	*Pooſh* or *Powſh*	December
10 *Makar*	*Māg*	January
11 *Koombha*	*Phāgan* or *Phalgoon*	February
12 *Meen*	*Chīt* or *Chītr*	March

According to the foregoing liſt, the Hindoo year begins with *Bīſāk* (anſwering to April from about the 10th of that month); and for this arrangement I have the authority of three almanacs, publiſhed at Calcutta, for different years, and by different perſons, as well as that of Mr. HALHED's tranſlation of a Code of Gentoo Laws, p. 24.; yet I obſerve that M. LE GENTIL, ABRAHAM ROGER, BESCHIUS, WALTHERUS, and all the miſſionaries, agree in placing the month

* The former is the common *Hindooſtanee*, the latter the *Sanſkreet* orthography: for both I am indebted to Mr. WILKINS.

Çhit,

Chit, or *Chitr*, at the head of the year (inſtead of the concluſion), and making it to correſpond with April. I can no way reconcile this incongruity, and only remark, that all the authorities for the one practice are from Bengal, and all thoſe for the other from the peninſula of India.

Lunar Conſtellations.

1	*Aſwenee*	15	*Swātee*
2	*Bharanee*	16	*Veeſakhā*
3	*Kreetteekā*	17	*Anooradhā*
4	*Robeenee*	18	*Jyēſhta*
5	*Mreegaſeerā*	19	*Moola*
6	*Adree*	20	*Poorvāſhārā*
7	*Poonarvaſoo*	21	*Oottarāſhārā*
8	*Pooſhwa*	22	*Abheejeet*
9	*Aſlēſhā*	23	*Sravana*
10	*Magha*	24	*Dhaneeſhthā*
11	*Poorva-phālgoonee*	25	*Satabheepā*
12	*Oottara-phālgoonee*	26	*Poorva-bhādrapad*
13	*Haſta*	27	*Oottara-bhādrapad*
14	*Cheetrā*	28	*Rēvatee*

The number of the lunar conſtellations is more uſually ſtated as amounting to twenty-ſeven only; and that which in the above liſt appears as the twenty-ſecond, is ſaid (in the *Ayeen Akbery*) to have been formed out of thoſe contiguous, for ſome particular purpoſe. As the moon's periodical revolution is performed in 27 days and part of the 28th, it is not ſurpriſing that this ambiguity with reſpect to the diviſion of the zodiac ſhould exiſt.

(*e*) Thus they ſay, "on the *full moon* in the (ſolar) month *Bhādoon*;" "on the moon's increaſe in the month *Sāwan*;" "on the firſt day of the *bright half* of the month *Bīſāk*." It is to be obſerved, that they term the ſemi-lunation from the change to the full, the *bright* half of the moon, becauſe it is above the horizon during thoſe hours wherein it is likely to be viſible. From the full to the change is called the *obſcure* half; becauſe, during the latter part, it does not riſe till the hours of reſt. See Beschius, Gram. Lat. Tam. p. 167. and Mr. Wilkins, Heetōpadēs, Note 70. to p. 43.

(*f*) Theſe lunar years do not appear to be ever taken collectively, or regarded in computations of time; and the months of which they conſiſt may not improperly be conſidered as a lunar diviſion of the ſolar year.

(*g*) P. du Champ (Bailly, Traité de l'Aſtronomie Indienne, p. 320.).

 (*h*) This

(*b*) This arrangement of the names of the days, ſo different from the order of the planets, is ingeniouſly accounted for by COSTARD, in his Hiſtory of Aſtronomy, p. 156. from the aſtrological appropriation of a planet to each of the 24 hours; but the reaſoning applies rather more directly to the Indian diviſion of the day into ſixty parts or hours. If we ſuppoſe, for example, that the ſun is made to preſide over the firſt hour of Sunday, Mars over the ſecond, Jupiter over the third, &c. in their natural order, we ſhall find the moon to anſwer to the firſt hour of Monday, Mars to the firſt of Tueſday, and ſo of the reſt. To make the rule apply to the European diviſion, we muſt reverſe the order, and reckon downwards from the ſun to Venus, Mercury, and the moon. The Hindoo names for the days of the week are properly as follows, though from the variety of appellations given to the planets, and particularly to the ſun and moon, they are ſometimes differently called.

Adeetya- *Ravee-* *Soorya-*	*-vara*	Sunday	Solis dies
Chandra- *Sōma-*	*-vara*	Monday	Lunæ
Mangala-	*vara*	Tueſday	Martis
Booddha-	*vara*	Wedneſday	Mercurii
Vreehaſpatee-	*vara*	Thurſday	Jovis
Sookra	*vara*	Friday	Veneris
Sannee-	*vara*	Saturday	Saturni

(*i*) At ſunriſe, according to M. LE GENTIL and others; but according to the *Soorya-Seedhanta* (before quoted), it is ſaid to have begun immediately after midnight. This treatiſe eſtimates alſo the diurnal revolutions from midnight, if we may credit the *Ayeen Akbery*, Vol. III. p. 14.

(*k*) "Les Indiens diſent qu'à l'inſtant du *Caliougam* il y a eu une conjonction generale de toutes les planètes; leurs tables en effet indiquent cette conjonction, et les nôtres montrent qu'elle a pu réellement avoir lieu." Traité de l'Aſtron. Ind. p. xxviii. "Le témoignage des Brames eſt ici appuyé ſur nos tables; et ce témoignage, qui n'a pu être fondé ſur un calcul, doit être dû à une obſervation réelle," p. 184.

(*l*) They frequently extract the ſquare root, apply the propoſition which is the 47th of EUCLID, and appear to have no mean knowledge of ſpherical trigonometry.

(*m*) The commencement of the *Kalee-Yoog* being calculated by the Hindoos according to the ſun's *mean* place, and the ordinary commencement of their years, according

according to his *true* place, it becomes neceſſary, when they compute the time elapſed from the beginning of their era, until that of any given year, to allow for the difference between the mean and true places at the former period, and on this account they deduct 2 d. 3 h. 32′ 30″ from the whole number of days.

(*n*) This is the common mode of ſpelling the name after the Perſian; according to the Sanſkreet orthography, it ſhould be *Veekramadeetya*. The place of his birth is ſaid to be *Toumma-naggree*, in the north-eaſtern part of *Malwa*. Prior to the era of *Bikramajit*, the Hindoos appear to have reckoned by that of *Judiſhter* (or *Yoodhe-ſhteera*), a king ſuppoſed to have reigned at *Haſtinapoor* about the commencement of the *Kalee-Yoog*. Of this era 3044 years are ſaid to have expired when the new reckoning was adopted; but the date is no other than that of the *Kalee-Yoog* itſelf, with the 3045th of which, as is ſeen by the table, the era of *Bikramajit* commenced. The tranſlation of the *Benares Pattra* makes the year of *Judiſhter*, 3044, to correſpond with the year 1784 of the Chriſtian era.

(*o*) See (*a*), latter part.

(*p*) The copper plate diſcovered at *Mongueer*, and tranſlated by Mr. Wilkins (Aſiatic Reſearches, Vol. I. p. 123), is dated in the 33d year of an era ſuppoſed to be that of *Bikramajit*, who is alluded to in the inſcription by an epithet ſignifying the *foe of Sakă*. For other ancient dates of this era, ſee Aſiatic Reſearches, p. 287. and 380. M. Bailly (Traité de l'Aſtr. Ind. p. cxxx.), quoting the authority of M. Anq. du Perron, places it 54 inſtead of 56 years before Chriſt.

(*q*) Phil. Tranſ. Vol. LXXVIII. p. 416.

(*r*) Take the two following paſſages as inſtances. "*Bikermajit* ruled for ſome time over the northern parts of the *Deccan*; but the *Rajahs*, headed by *Salbahan*, having revolted, they gave him battle, and he was ſlain." F. Wilford, Aſiat. Reſ. p. 374. "Bikarmadjit tint ſa cour à *Oudjen*; c'eſt à lui que commence l'ere Indienne. Il fut vaincu et tué dans une battaille par *Salbhân* roi *Patane*." Bernoulli (P. Tieffenthaler), Vol. I. p. 357. The confuſion ariſing from this anachroniſm is increaſed by an ambiguity of names; for that of *Sakă* or *Sak*, which belonged to the predeceſſor of Bikramajit, is likewiſe a name by which *Salabân* is commonly known, and from which his era is called the *Sak-âbda*.

For the relative date of the era of Bikramajit, I have the authority of Mr. Wilkins (Aſiat. Reſ. Vol. I. p. 130.), who makes 1837 correſpond with 4882 of the *Kalee-Yoog*, and with 1703 of Salabân. Theſe are corroborated in a manner that puts their accuracy out of diſpute.

(*s*) The manner of dating appears thus in a grant of land, of which a tranſlation is given in the Aſiatic Reſearches, Vol. I. p. 363. "On the fifteenth of the

the bright moon of *Cartica*, in the middle of the year *Pingala*, when nine hundred and forty years, ſave one, are reckoned as paſt from the time of king *Sáca*, or in figures, the year 939, the moon being then full and eclipſed."

The names by which the years of the cycle are diſtinguiſhed, and of which the ſignifications relate to the ſun, as having a good or evil influence, or to the year, as being more or leſs lucky, are as follows:

1 Prabhava	21 Sarvajeet	41 Plavanga
2 Veebhava	22 Sarvadharee	42 Keelaka
3 Sookla	23 Veerooddhee	43 Sowmya
4 Parama-jyōtee	24 Veekreetee	44 Sādhārana
5 Projōtpatee	25 Kara	45 Veerōdhakreet
6 Anggeeraſa	26 Nandana	46 Pareetāpee
7 Sree-mookha	27 Veeſhya	47 Pramadeechā
8 Bhava	28 Jaya	48 Ānanda
9 Yoova	29 Manmatha	49 Rakſhaſa
10 Dhātoo	30 Doormookha	50 Nala
11 Eeſwara	31 Aveelambee	51 Peengala
12 Bahoodhānya	32 Veelambee	52 Kala-yooktee
13 Pramadee	33 Veekāree	53 Sceddhārthee
14 Veekrama	34 Servaree	54 Rowdree
15 Veeſhoo	35 Plava	55 Doormatee
16 Cheetrabhānoo	36 Soobha-kreet	56 Doondoobhee
17 Soobhānoo	37 Sōbhā-kreet	57 Roodrādheekāree
18 Tarana	38 Krōdhee	58 Raktākſhee
19 Pārtheeva	39 Veeſwavaſoo	59 Krodhana
20 Vyaya	40 Parābhava	60 Akſhaya

(*t*) At the commencement of the era of *Salabân*, 49 years were wanting to complete the current cycle; and, in calculations where it is neceſſary to adjuſt the two reckonings, this number muſt be attended to; but as the object to be known is not the number of cycles, but the number of the year of an individual cycle, or the correſponding year of the era, it is indifferent whether in their additions and ſubtractions they make uſe of 49 years ſimply, or of 49 added to one or more integral cycles, as 109, 349, or 409, which produce the ſame reſult. This accounts for the difference of practice remarked by M. BAILLY (p. 51.) in the Brahmans of different cities, and it ſeems to have no relation to the origin of the cycle.

(*u*) M. LE GENTIL, Voyage dans les Mers de l'Inde, Vol. I. p. 240—5. In the common notes of an almanac, publiſhed at *Calcutta*, by Mr. REUBEN BUR-

now, the year 1784 is made to correſpond with the years 48 and 49 (or *Anund* and *Rakſheſa)* of the cycle of ſixty, which appears to have the ſanction of a Preface to the Benares *Pattra*, or calendar, of which a tranſlation is given. Though reluctant to call ſuch authority in queſtion, I venture to aſſert, that the true Hindoo date ſhould be 37 and 38, for the former and latter part of 1784. From among a variety of authorities concurring to eſtabliſh this computation, I ſhall produce, in the firſt place, that of Beſchius, the Author of a Grammar of the *Tamool* language (peculiar to that country in which the cycle is uſed), who has treated briefly, but with great accuracy, of chronology, and given the names of the 60 years in the original character, with the correſponding dates. M. LE GENTIL mentions having poſſeſſed himſelf of a copy of this work (printed at Tranquebar in 1738), and bringing it with him to Europe. ABRAHAM ROGER (whoſe book, entituled, in the French tranſlation from the Dutch, *De la Vie et des Mœurs des Brames*, and printed at Amſterdam in 1670, contains much original information reſpecting the Hindoos) gives likewiſe the names of the ſixty years; and ſays (p. 79.), that the year 1640, in the month of Auguſt, correſponded with *Wicrama*, the 14th of the cycle; conſequently 1760 with the 14th alſo, and 1784 with the 38th. The communications of WALTHERUS, a learned miſſionary, publiſhed by BAYER under the title of *Doctrina temporum Indica*, are explicit as to this point. M. BAILLY, in his *Traité de l'Aſtronomie Indienne*, adopts in his calculations the date that I have aſſigned, and gives (p. 326.) the years of the cycle of ſixty from P. DU CHAMP, in which 1724 (and conſequently 1784) is remarked as correſponding with the 38th year. To this, if it were neceſſary, I might add the authority of a *Hindoo date* in the Aſiatic Reſearches, before quoted, where the year *Pingala* (51) agrees with 939 of *Saca* or *Salabân*. The year 1 of the preſent cycle correſponded with 1669 of the era, according to the table I have given; and therefore 51 of the preceding cycle with 1659, from which deduct twelve whole cycles, or 720 years, and 939 remain; which, as the table was founded on other documents, will be allowed no weak corroboration, the date in queſtion being from an ancient copper plate. I obſerve, with ſome concern, that I differ alſo from Mr. BURROW, both in the year of *Bikramajit* and that of *Salabân*. He makes the interval between them 136 years, whereas, by the authorities I have followed, it ſhould be only 134, or 56 years before Chriſt, and 78 after. I ſhould certainly not be forward to point out theſe apparent inaccuracies but from an apprehenſion, were I to paſs them unnoticed, that my own computations might be condemned on an appeal to thoſe of Mr. BURROW's almanac as a criterion. I have not ſeen any ſubſequent to that for 1784, which was the firſt.

(*v*) On reference to the *Ayeen Akbery*, or Inſtitutes of the emperor *Akbar*, I find mention made (Vol. I. p. 344.) of an era eſtabliſhed by him, being that of the commencement of his reign, in the year of Chriſt 1556. Although nothing is ſaid of the conformity of its date with the current year of the *Hejera*, yet it is probable that this was the origin of what has ſince been called the Bengal era.

In the province of *Behar*, and ſome other parts of the empire, a year was eſtabliſhed with a view to the revenue ſolely, called the *Fuſſullee* year. It appears to begin, and to take its date, from the month of September preceding the commencement of the common Bengal year, and therefore 1198 ſhould begin in September 1790. Mention is made (in the Alph. Grandonico-Malabaricum, Romæ, 1772) of an era employed in the ſouthern extremity of the peninſula, of which 965 ſhould correſpond with the year of Chriſt 1790.

(*w*) Mem. Acad. des Sciences, Tom. VIII. p. 218. Traité de l'Aſtronomie Ind. p. 18. and 21.

(*x*) This agrees with the Hindoo year given in the *Soorya Seedhanta*, and differs 6″ from that by M. LE GENTIL. Theſe ſmall differences ariſe probably from the forms of calculation, and are not fundamental.

(*y*) The cycle of the Chineſe and Tartars comprehends 60 years, expreſſed by an artificial arrangement of 10 words or particles, prefixed to the names of 12 animals, in ſuch a manner that 6 repetitions of the former coincide with 5 repetitions of the latter, and bring them to the ſame relative ſituation at the recommencement of the cycle.

(*z*) LOUBERE, Relation of Siam, Eng. tranſ. 1693, p. 169. et 202. Journal du Voyage de Siam, par M. L. D. C., Par. 1687, p. 286. Hiſtoire Nat. et Pol. du Royaume de Siam, par N. Gervaiſe, Par. 1688, p. 154, 155.

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APPENDIX.

OUR late much reſpected Colleague, Major-General Roy, having finiſhed, in September 1788, the trigonometrical meaſurement deſcribed in the firſt Part of this Volume, returned to London in a very indifferent ſtate of health. From this time he employed all the leiſure that his illneſs, and his various official avocations, allowed, in preparing the account of his operations, to be laid before the Royal Society. But toward the autumn of 1789 his infirmities encreaſed ſo much, that the medical Gentlemen he conſulted adviſed him to ſpend the following winter at Liſbon, for which place he accordingly embarked in the beginning of November. Previous to this, however, he finiſhed the firſt Copy of his Paper; but it was much hurried toward the latter part, and not rendered ſo perfect as the General would undoubtedly have made it with more time and better health. He returned to England in April 1790, and the Paper was ſent to the preſs before the end of the ſame month. Unfortunately the General did not live to ſee the printing quite completed; he corrected, indeed, all the ſheets except the three laſt; but without comparing his manuſcript copy with the original papers and obſervations. Several errors which had been diſcovered in the courſe of the printing, toge-

ther

ther with the obſcurity of the account in certain parts, induced ſome of the General's Friends, Members of the Royal Society, to requeſt, after his deceaſe, that the whole might be reviſed by a competent perſon, who ſhould compare it with the original documents, correct ſuch miſtakes as might be diſcovered, and illuſtrate whatever required further explanation. No one could be found ſo proper for this taſk as Mr. DALBY, the Gentleman of whom the General makes ſuch honourable mention in his Paper, and who, having aſſiſted in all the operations, was as well acquainted with every part of them as the General himſelf. The reſult of Mr. DALBY's examination is the following Remarks; which being much too long for inſertion in the liſt of *errata* (where only the errors of the preſs are noticed) is here added ſeparately, by way of Appendix.

C. BLAGDEN.

Remarks on Major-General Roy's *Account of the Trigonometrical Operation, from* Page 111. *to* Page 270. *of this Volume. By Mr.* Iſaac Dalby.

PAGE 134. l. 20, &c. The inclinations of the baſes with the meridians were determined by ſpherical computation, and therefore can only be conſidered as *nearly* true.

P. 171. l. 6. from bottom, *for* we have area in feet *put* we have log. area in feet.

P. 173. in the VIII triangle, *for* 0.01 *put* 0.1.

P. 174. in the IX triangle, *for* 0.88 *put* 0.83.

P. 175. in the XVII triangle, *for* 71855 *put* 71885.

P. 177. This method of making the compariſon on a long diſtance, when the meaſured baſes are ſhort, and nearly of the ſame length, ſeems preferable to that of carrying the computation directly from one baſe to the other. Determinations of this kind, however, muſt always be uncertain to particular limits on account of the inaccuracy of inſtruments and obſervations combined with the unknown figure and dimenſions of the earth. Was the earth a ſphere of a known magnitude, the moſt natural method of computation would have been by ſpherical trigonometry, after the obſerved angles had been corrected for that purpoſe; which method (ſuppoſing the angles wanted no correction, or each had been accurately obſerved) would ſhew which baſe was meaſured neareſt the truth. Or the ſame thing might alſo be obtained by plane trigonome-

try, uſing the chords of the meaſured baſes, and the angles formed by the chords of the other ſides of the triangles (which angles might then be found from the horizontal ones) inſtead of the obſerved angles.

In the application of plane trigonometry to the obſervations, that part of the earth's ſurface to which the operation has been confined, is conſidered as a plane, and the meaſured baſes as right lines on that plane; but whether the computations are made on this principle, or attempted on that of taking the baſes and the other ſides of the triangles as chords, there ſeems to be no certain rule for reducing the obſerved angles of each triangle to 180°, ſo as to give the diſtances the moſt correct, which would alſo be the caſe if the angles of each triangle had been taken in the ſame plane; hence it is evident, that the method of correction has been in ſome degree arbitrary; for, though the ſum of the three obſerved angles of each triangle is in general very near what it ought to be (taking the earth as a ſphere); yet, when that ſum is not exactly 180°, in reducing them to plane ones, each obſerved angle may be taken as a plane one, and the other two augmented in caſe of a defect; but each ought to be diminiſhed when there is an exceſs in the obſerved ſum. In making theſe reductions, however, it is evidently neceſſary to conſider whether each of the obſerved angles is equally well aſcertained, and correct them accordingly; but this muſt be left to the judgement of the obſerver.

From the foregoing conſiderations, it follows, that the angles of the triangles taken as plane ones may be varied to certain limits, and conſequently the oppoſite ſides deduced therefrom muſt vary to certain limits alſo; but it is evident, that a mean of the extreme reſults, obtained in this manner, will be very near the truth; and therefore this method of making the

compariſon

compariſon ſeems leſs liable to objection than that by a ſingle correction of the ſame angles. Accordingly, if we vary the angles (in reducing them to 180°) from Hounſlow Heath to the XIII triangle, ſo as to produce the greateſt and leaſt effects on the lengths of the oppoſite ſides, there will reſult 141750½ and 141746½ feet, nearly, for the greateſt and leaſt, and 141748½ for the mean diſtance of Fairlight and Hollingborn. In like manner, the baſe on Romney Marſh will give 141745.6 and 141744.4 feet, nearly, and the mean 141745 feet, for the diſtance of the ſame ſtations; the difference in the mean reſults is 3½ feet on a diſtance of near 27 miles; and therefore the baſe on Hounſlow Heath meaſures the other, by theſe determinations, to about 9 inches; and, becauſe the latter baſe is the longeſt, it would meaſure the former on Hounſlow Heath to ſomething leſs.

The diſtance of the ſtations of Fairlight and Hollingborn in the XIII triangle is 141747.1 feet, and from this all the diſtances to the eaſtward are computed; but if the baſes are meaſured equally exact, the diſtance of the above ſtations, or 141745 feet, determined from the baſe on Romney Marſh, muſt be more correct than the other, becauſe the connection of Fairlight and Hollingborn with this baſe is formed by three or four triangles only, whereas on the other ſide, the computation runs through eight or nine. The difference, however, is but 2 feet, and that in an extent of almoſt 27 miles, which will make about 7½ feet leſs for the diſtance between the meridians of Greenwich and Paris.

Among the angles corrected for computation, it will be perceived, that ſometimes the quantity of an angle ſeems not to be exactly what the obſerved angle ought to give. In theſe caſes the obſerved angle is leſs to be depended on than the

others of the ſame triangle: for inſtance, in the firſt triangle the obſerved angle at Hanger Hill is 42° 2′ 32″, and that for computation 42° 2′ 34″; now this angle was not taken ſo accurately as could be wiſhed, but the others were repeatedly obſerved.

P. 178. in the XXIX triangle, *for* 186113 *put* 147386.9.

P. 180. in the XXXIV. triangle, *for* 252469.9 *put* 252496.9.

As the obſervations at Fairlight and Dover are omitted, upon which the angles of the XXXIII and XXXIV triangles depend, it will be neceſſary to give them, and alſo ſhew the manner of obtaining theſe angles.

	°	′	″
At Dover Caſtle, the angle between the white light at Montlambert and the lamp at Padleſworth was obſerved	109	8	25.5
Corrected for computation . . .	109	8	25
At Fairlight, the angle between the white lights at Montlambert and Blancnez was obſerved	17	46	5
For computation . . .	17	46	3.5
Between the lamp at Lydd and white light at Blancnez	18	2	31
For computation . . .	18	2	31.5

The acute angles in the XXXII triangle reſult from the other angle and including ſides 147386.9 and 42561.18.

		°	′	″
Angle at Fairlight	in the XXIX triangle	13	38	2.95
	in the XXXII triangle	6	6	39.43
Angle at Fairlight between Dover and Lydd		7	31	23.52
Angle at Fairlight between Lydd and Montlambert (17° 46′ 3″½ + 18° 2′ 31″½) .		35	48	35
Angle at Fairlight in the XXXIII triangle		43	19	58.52

	°	′	″
Angle at Dover between Padleſworth and Montlambert . . .	109	8	25
—— at Dover in the XXXII triangle	21	37	55·42
Angle at Dover in the XXXIII triangle .	87	30	29.58

The third angle, or 49° 9′ 31″.9 at Montlambert, is the ſupplemental one.

If from the angle at Fairlight in this triangle we take 17° 46′ 3″½ we have 25° 33′ 55″.02 for the angle at Fairlight in the XXXIV triangle; and if to 87° 30′ 29″.58 we add 23° 25′ 0″.25 (the angle at Dover in the XXXV triangle) it gives 110° 55′ 29″.83 for the angle at Dover; that at Blancnez is the ſupplemental one.

The ſituation of the ſtation at Montlambert, as determined by the obſervations made on this ſide of the Channel, has not however totally depended on thoſe made at Fairlight and Dover; another obſervation at Padleſworth has been uſed by way of check, or verification. This was made in a very favourable ſtate of the air, when the angle between the flag-ſtaff at Dover and maſt at Montlambert was found to be 58° 27′ 11″½, which is nearly what reſults from computation; for 42561.18 and 168821.07 feet, the reſpective diſtances of Dover from Padleſworth and Montlambert, with the included angle 109° 8′ 25″, give this angle 58° 27′ 10″.9.

It ought to be remarked, that the angle at Blancnez 119° 41′ 28″.9, communicated by M. CASSINI, is an horizontal one; that of the XXXV triangle, or 119° 41′ 41″.6, is the reſult of a computation by plane trigonometry, which, if accurate, ſhould be leſs than the horizontal one at the ſame point, and therefore the *maximum* of the difference muſt be ſomewhat greater than 12″.7.

P.

P. 182. &c. The triangles after the XXXVI, and what follows to the end of the article, do not ſeem neceſſary, on our part, to complete the triangular connection between Greenwich and Paris, becauſe it may be done in the following manner from the triangle formed by Dover, Calais, and Dunkirk. In this triangle, the ſide between Dover and Calais is 137449.9 feet (ſee the XXXVI triangle); and by M. CASSINI's Paper, communicated in 1789, it appears, that the ſide between Calais and Dunkirk is 19349.34 toiſes (=123729.3 feet), and the included angle at Calais 139° 17′ 35″.6; theſe give 19° 14′ 13″.1 and 21° 28′ 11″.3 for the other two angles, and 244919 feet for the diſtance of Dover and Dunkirk; alſo 28232.7 for what Dunkirk is ſouthward, and 243287 feet for what it is eaſtward, from Dover: this laſt added to 307366.8, the diſtance of Dover from the meridian of Greenwich, gives 547053.8 feet for what Dunkirk is eaſt from the meridian of Greenwich on a parallel to the perpendicular; but the length of the arc of the great circle which paſſes through Dunkirk, and is perpendicular to the meridian of Greenwich, is very near the ſame as the length of this parallel, or 547053.8 feet (though accurately ſomewhat leſs); hence, if we take 61247½ fath. = 1°, we have 1° 29′ 19″.1 for that arc, and the latitude of Dunkirk being 51° 2′ 9″.3 (See Sect. VI. Art. 12.); therefore, coſine 51° 2′ 9″.3 : rad. :: ſine 1° 29′ 19″.1 : ſine 2° 22′ 3″.8 the longitude of Dunkirk (agreeing with that in Art. 12.); and rad. : tang. 51° 2′ 9″.3 :: tang. 1° 29′ 19″.1 : coſine 88° 9′ 34″ for the other angle of this triangle of which the co-latitude of Dunkirk is the hypothenuſe.

Dunkirk is eaſt from the meridian of Paris 1416, or 1430 toiſes (ſee Art. 12. Sect. VI.), a mean of theſe give 1′ 29″.14 as an arc of a great circle; this, with the co-latitude of Dun-

kirk for the hypothenuſe, will give 89° 58′ 10″ for the angle between the meridian and this arc; hence 89° 58′ 10″ − 88° 9′ 34″ = 1° 48′ 36″ is the angle at Dunkirk between the two arcs; one perpendicular to the meridian of Greenwich, and the other to that of Paris; therefore, if we take 1416, or 1430 toiſes, as the leg of a plane triangle adjacent to this angle, we get 9059, or 9148½ feet, for the diſtance of the meridian of Paris from Dunkirk on a great circle perpendicular to the meridian of Greenwich; theſe taken from 547053.8 will leave 537994.8 or 537905.3 feet, for the diſtance between the meridians of Greenwich and Paris on that circle, according as Dunkirk is 1416 or 1430 toiſes from the meridian of Paris.

Should it be thought more accurate to make uſe of the diſtance between Calais and Dunkirk, according to the ſcale in the XXXV triangle deduced from the Engliſh obſervations acroſs the Channel, it is had at one proportion thus; as 12077.85 toiſes (the French diſtance between Blancnez and Montlambert) is to 77235 feet (the Engliſh diſtance) ſo is 19349.34 toiſes (the French diſtance of Calais and Dunkirk) to 123734.5 feet, as in the XL triangle, which exceeds the French diſtance about 5 feet: this will give the diſtance between the meridians of Grenwich and Paris 4.4 feet more than the above determination. But was the baſe on Romney Marſh adhered to, it would give the diſtance about 3 feet leſs; and therefore the reſults of the French triangles on their coaſt would agree nearer with the deductions from this baſe than from the other on Hounſlow Heath.

P. 187. in lines 1. and 8. *for* 358.6 *put* 349.4; and conſequently in line 10. *for* 133409.8 *put* 133419. This error is the cauſe of the difference in the diſtances of the parallels of latitude of Greenwich and Paris as given in Art. 10. and 13. Sect. VI.:

for,

for, in pages 185. 187. we have 133746.3 and 133768.4 fath. the diſtances of Dunkirk north from Paris, the mean is 133757.4; if from this we take 358.6, we have 133398.8, as in Art. 10.; but ſubtracting 349.4, gives 133408 for what M is north of Paris, which added to 27248.2 there reſults 160656.2 agreeing with that in Art. 13.

P. 190, 191. It will immediately be perceived, that the columns in the table of general reſults here alluded to, have been filled by a method ſimilar to that of working a *traverſe*. The following table, however, was previouſly drawn up to facilitate the computations, and by which the numbers in the two firſt columns of diſtances may at any time be eaſily examined.

This table will readily be underſtood; for, if we ſuppoſe parallels to the meridian of Greenwich to be drawn through the ſtations on the left, oppoſite on the right are the angles which the adjacent ſtations make with theſe parallels.

Greenwich Obſ.	Norwood .	38°	7′	16″	SW
Norwood .	Greenwich Obſ.	38	7	16	NE
	Hundred Acres	42	22	39.2	SW
	Hanger Hill .	49	31	22.7	NW
Hundred Acres .	Norwood .	42	22	39.2	NE
	Hanger Hill .	19	50	1.7	NW
	St. Ann's Hill .	73	48	38.3	NW
Hanger Hill .	Norwood .	49	31	22.7	SE
	Hundred Acres	19	50	1.7	SE
	Hampton Poor-houſe	23	30	53.4	SE
	St. Ann's Hill .	48	34	42.2	SW
	King's Arbour .	65	33	27.4	SW

Hampton

		°	′	″	
Hampton Poor-houſe	Hanger Hill .	23	30	53.4	NE
	King's Arbour .	44	24	45.6	NW
	St. Ann's Hill .	74	8	40.9	SW
King's Arbour .	Hanger Hill .	65	33	27.4	NE
	Hampton Poor-houſe	44	24	45.6	SE
	St. Ann's Hill .	29	49	49.4	NW
	Windſor .	87	29	43.1	NW
St. Ann's Hill .	King's Arbour .	29	49	49.4	NE
	Hanger Hill .	48	34	42.2	NE
	Hampton Poor-houſe	74	8	40.9	NE
	Hundred Acres .	73	48	38.3	SE
	Windſor .	29	19	24.6	NW
Greenwich Obſ. .	Severndroog Caſtle	73	49	34	SE
Severndroog Caſtle	Greenwich Obſ.	73	49	34	NW
	Botley Hill .	11	23	18.5	SW
	Wrotham Hill .	46	18	30	SE
Botley Hill .	Severndroog Caſtle	11	23	18.5	NE
	Wrotham Hill :	79	16	28.7	NE
	Goudhurſt .	60	38	49.3	SE
	Frant .	43	28	20.3	SE
Frant .	Botley Hill .	43	28	20.3	NW
	Wrotham Hill .	6	50	57.7	NE
	Hollingborn Hill	55	19	35.3	NE
	Goudhurſt .	82	24	11.4	NE
	Fairlight Down	45	17	22.7	SE
Wrotham Hill .	Severndroog Caſtle	46	18	30	NW
	Botley Hill .	79	16	28.7	SW
	Frant .	6	50	57.7	SW
	Goudhurſt .	25	12	14.7	SE
	Hollingborn Hill	77	21	25.7	SE

		°	′	″	
Goudhurſt .	Hollingborn Hill	38	51	47.3	NE
	Tenterden .	72	54	53.3	SE
	Fairlight Down	23	15	17.5	SE
	Frant .	82	24	11.4	SW
	Botley Hill .	60	38	49.3	NW
	Wrotham Hill .	25	12	14.7	NW
Fairlight Down .	Frant .	45	17	22.7	NW
	Goudhurſt .	23	15	17.5	NW
	Hollingborn Hill	3	8	32.3	NE
	Tenterden .	12	5	40.9	NE
	Allington Knoll	45	46	21.3	NE
	Lydd .	67	4	58.3	NE
	Blancnez .	85	7	29.7	NE
	Montlambert .	77	6	26.7	SE
Hollingborn Hill	Wrotham Hill	77	21	25.7	NW
	Frant .	55	19	35.3	SW
	Goudhurſt .	38	51	47.3	SW
	Fairlight Down .	3	8	32.3	SW
	Tenterden .	5	46	56.8	SE
	Allington Knoll	45	47	55.7	SE
Tenterden Church	Hollingborn Hill	5	46	56.8	NW
	Goudhurſt .	72	54	53.3	NW
	Fairlight Down	12	5	40.9	SW
	Lydd .	50	27	11.6	SE
	Allington Knoll	85	47	25.3	NE
Lydd Church .	Fairlight Down	67	4	58.3	SW
	Tenterden .	50	27	11.6	NW
	Ruckinge .	6	16	20.4	NW
	Allington Knoll	12	46	58.3	NE
	High Nook .	37	4	28.1	NE
	Padleſworth .	41	10	52.6	NE
	Folkſtone Turnpike	50	49	22	NE

Ruckinge

		°	′	″	
Ruckinge .	Lydd .	6	16	20.4	SE
	High Nook .	55	15	9.8	SE
	Allington Knoll	70	25	31.6	NE
Allington Knoll .	Hollingborn Hill	45	47	55.7	NW
	Tenterden .	85	47	25.3	SW
	Fairlight Down	45	46	21.3	SW
	Lydd .	12	46	58.3	SW
	High Nook .	21	1	47.8	SE
	Folkſtone Turnpike	82	56	18.9	NE
High Nook .	Allington Knoll	21	1	47.8	NW
	Ruckinge .	55	15	9.8	NW
	Lydd .	37	4	28.1	SW
	Folkſtone Turnpike	58	39	12.6	NE
	Padleſworth .	43	50	47.1	NE
Padleſworth .	High Nook .	43	50	47.1	SW
	Lydd .	41	10	52.6	SW
	Folkſtone Turnpike	64	18	47.4	SE
	Dover Caſtle .	81	11	30.1	NE
	Swingfield .	44	47	7.1	NE
Folkſtone Turnpike	Lydd .	50	49	22	SW
	High Nook .	58	39	12.6	SW
	Allington Knoll	82	56	18.9	SW
	Padleſworth .	64	18	47.4	NE
	Swingfield .	3	51	7.8	NW
	Dover Caſtle .	65	52	45.6	NE
Swingfield .	Padleſworth .	44	47	7.2	SW
	Folkſtone Turnpike	3	51	7.8	SE
	Dover Caſtle .	79	27	47.8	SE

Dover Caſtle .	Swingfield	79°	27′	47″.8	NW
	Padleſworth	81	11	30.1	SW
	Folkſtone Turnpike	65	52	45.6	SW
	Montlambert	27	56	54.8	SE
	Blancnez .	51	21	55.1	SE
	Calais .	64	8	37.1	SE
	Dunkirk .	83	22	52.9	SE
	—— by M. CASSINI's diſtance of Dunkirk and Calais	83	22	50.2	
	Point M .	82	33	14.4	SE

The difference between the complement of 82° 33′ 14″.4 and the angle at M in the XL triangle is 14° 51′ 3″.9, the angle RMC referred to in Art. 10. and 11. Sect. VI.

P. 194. It is ſaid, that the angle AB*v* (Pl. X. fig. 2.) is equal to the angle BA*r*, and conſequently at p. 199. that the ſum of the obſerved angles PAB, PBA, are equal to the ſum that would be found on a ſphere. This (though extremely near in any of the ſpheroids hitherto aſſumed for the figure of the earth) is not geometrically true when the points of obſervation are *on* the ſurface of the ſpheroid, and each angle taken exactly in the plane of the horizon. For, it is evident, that to have the ſum *accurately* the ſame, the points A, B (the places of obſervation) muſt be at equal diſtances from G and W; and therefore, if at any two points thus taken in the verticals GA, WB, the angles are ſuppoſed to be in planes parallel to the reſpective horizons at A and B, their ſum will always be the ſame. Hence, becauſe the vertical WB is greater than GA, if the angles are accurately horizontal ones at A and B, their ſum muſt be *greater* on the north ſide, and *leſs* on the ſouth,

than

than on a ſphere, except the latitudes of A and B are the ſame. The difference, however, is ſo minute, that for practical purpoſes they may be conſidered as equal (as in this Section and the corollary, p. 215.), without ſenſible error. In the example at p. 196. the difference in the ſum of the horizontal angles at A and B on this ſpheroid, and on a ſphere, is a ſmall fraction of a ſecond; but it requires a nice computation to diſcover the exact quantity. The method, however, is to compute the angle at B in the ſame manner as that is done at A; or by taking the point of obſervation in the vertical GA produced, 168 fathoms (the difference of the verticals WB, GA) above the ſurface at A, and determining the diminution in the horizontal angle by a re-computation.

By purſuing a method of computation ſimilar to that for the point A at p. 196. it is evident, that the three horizontal angles of any triangle on a known ſpheroid may be determined.

P. 195. bottom line, *for* AGH *put* AGK.

P. 203. There ſeems a miſtake towards the latter part of this page; becauſe it will be ſeen, that no ſuch ſpherical triangles have been uſed in the computations but in Art. III. p. 206.

P. 205. l. 13. from bottom. This muſt allude to one place of obſervation only; becauſe in this operation (where the latitudes have not been obſerved) a principal advantage lies in having one of the ſtations (like Botley Hill) on, or near the meridian of Greenwich, on account of obtaining its latitude pretty exact; but the farther off the other place of obſervation is, the better it is for the purpoſe.

P. 207. In l. 14. from bottom, *for* :: P* *put* :: ſine P*.

P. 208. from l. 5. to the period in l. 12. from bottom, ſhould run thus:

If

If the latitude of the point B was given, and the earth a ſphere, the co-latitude BP and the obſerved angles PBG= 119° 21′ 13″.2, and PGB=60° 17′ 15″.7, would give PG the co-latitude of G, and the angle BPG the difference of longitude of B and G.

Taking a ſphere whoſe diameter is nearly a mean between thoſe in M. BOUGUER's ſpheroid, the length of a degree of a great circle is 60859.1 fathoms, and the latitude of B will be 51° 16′ 41″.45; therefore BP=38° 43′ 18″.54; this, with the obſerved angles at B and G, give PG=38° 53′ 6″.72, and the angle BPG, or difference of longitude=27′ 36″.7; therefore in the right-angled ſpherical triangle PRG, rad. : tang. GP :: coſine angle RPG : tang. 38° 53′ 3″.47=RP; and rad. : ſine GP :: ſine RPG : ſine 17′ 20″=RG.

P. 209. l. 9. *for* 51° 16′ 46″ *put* 51° 16′ 46″.1.

P. 213. Correct the title of this Article, by reading *geodetical meaſurement* for "*pole-ſtar obſervations*," in the firſt line.

P. 217. After the word "meridian", in the third line of Art. X. inſtead of "and alſo the differences of latitude and longitude have been obtained by very accurate obſervations of the pole-ſtar made at certain ſtations to the eaſtward of Greenwich,", *read* and alſo the difference of longitude between Botley Hill and Goudhurſt have been obtained by obſervations of the pole-ſtar.—A correction of this kind ſeems neceſſary, becauſe the pole-ſtar obſervations have not been uſed in finding the differences of latitude. From the directions of the meridians at the above ſtations, the value (in parts of a degree) of the meaſured arc of a great circle, perpendicular to the meridian, has been determined; hence the lengths of the degrees in the Table, p. 227. have been inferred. The diſtances from

the meridian of Greenwich (in the Table of General Reſults) have been converted into degrees, &c. according to this Table; and the others from the perpendicular in the next column, according to M. BOUGUER's ſcale on the meridian (which is had ſufficiently accurate from the Table, p. 298. *Fig. de la T.* or that at p. 228. Phil. Tranſ. 1787, by an eaſy approximation) theſe meridional arcs applied to the co-latitude of Greenwich, with the other arcs perpendicular to the meridian, form the legs of the triangles by which the latitudes and longitudes of the ſtations have been computed. The meridional arcs, however, have been corrected, as in the example in this Article (where the value of Rr has been added) when the diſtances of the ſtations from the meridian of Greenwich are conſiderable.

In determining the latitude of M in this Article, a ſpheroidical correction has been applied to the reſult by ſpherical trigonometry, as in Art. VII. but that computation is made on a figure of known dimenſions, and conſequently the latitude of *r* (fig. 7.) is given; but it does not follow, that the true latitude of *r* (fig. 10.) would exactly correſpond with M. BOUGUER's hypotheſis, though the length of the whole meridional arc between Greenwich and Paris is found to agree extremely near; and therefore no correction of this kind is applied to the other latitudes in the Table of General Reſults.

The greateſt accuracy, however, is abſolutely neceſſary in determining the directions of the meridians if we would derive ſatisfactory concluſions therefrom, when the places of obſervation are obliquely ſituated with reſpect to the meridian, and at a diſtance from each other not greater than that between Botley Hill and Goudhurſt, becauſe an error of $1''$ in the horizontal angle at either of theſe places will produce an error in their

difference

difference of longitude of 1″.2 of a degree, and conſequently a variation of about 6″ in the longitude of Dunkirk or Paris.

Was the diſtance of the ſtations about 36 miles, the error in longitude would be the ſame as that in the horizontal angle, or 1″.

The length of the arc RG (Pl. X. fig. 5.) is 17695 fathoms, and its value 17′ 20″.06 as an arc of a great circle perpendicular to the meridian. Now, was the earth a ſphere, the length of any arc, would be to the number of degrees it contained, as 17695 to 17′ 20″.06; but this is not accurately the caſe on a ſpheroid; though, on this account only, the error in longitude (which is in defect) deduced from an arc of a great circle obtained in the above manner, muſt be ſmall to the extent of 3 or 4 degrees (in the latitudes of the places of obſervation) on a ſpheroid not more oblate than the earth.

It may be obſerved, that in determining the differences of longitude by the pole-ſtar obſervations, the ſtations ſhould be as nearly eaſt and weſt from each other as the nature of the country will permit, becauſe in that direction, any errors which may be thought to ariſe from the uncertain inclination of the verticals on the ſpheroid, will vaniſh; and, what is of more conſequence, a longer arc of a great circle perpendicular to the meridian will thereby be determined than could be in any other direction with the ſame diſtance. On this account the ſtations at Botley Hill and Hollingborn Hill (for one is ſeen from the other) are eligible. Their diſtance is about 28½ miles, which would meaſure near 24′ of a degree of a great circle perpendicular to the meridian.

P. 220. Art. XI. ſeems to want correction: for, if Mg is a leſſer circle parallel to the meridian GR, it will cut the great circles *r*M, Gg, at right angles. Hence, RMC – RM*r*

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$(14° 51' 3''.9 - 19''.42) = 14° 50' 44''.5 = rMC$, which added to 90° ($rMg$) gives 104° 50′ 44″·5 for the angle gMC; from this take 1° 48′ 38″.6 (PMg), and we have 103° 2′ 5″.9 for the angle PMC deduced according to this method. But it cannot be ſaid to reſult from the Britiſh obſervations, becauſe the French angles were made uſe of to the eaſtward of Dover for obtaining the angle RMC, on which it depends.

P. 227. The lengths of the degrees of longitude in the Table were found thus: as radius : coſine of the latitude :: length of a degree of a great circle perpendicular to the meridian : length of a degree of longitude. This proportion is true on a ſphere, but not accurately ſo on a ſpheroid.

P. 229. *for* 43° 39′ *put* 48° 39′, the latitude of St. Malo.

As the new longitudes in this Table have not all been obtained in the ſame manner, it may not be improper to give the methods of computation.

The latitude of Straſbourg (*Deſcrip. Geom.*) is 48° 34′ 50″, and its diſtance from the meridian of Paris 204779 toiſes (=218243.17 fathoms) which, if we take 61225 fathoms = 1° (ſee the Table, p. 227.) gives 3° 33′ 52″.6; hence, as coſine 48° 34′ 50″ : rad. :: ſine 3° 33′ 52″.6 : ſine 5° 23′ 33″ the longitude.

In the *Connoiſſance des Temps* 1788, the latitude of Straſbourg is 48° 34′ 35″, longitude 5° 26′ 18″; therefore, as rad. : coſine 48° 34′ 35″ :: ſine 5° 26′ 18″ : ſine 3° 35′ 42″.3, the arc of the great circle (from which its longitude was computed) paſſing through Straſbourg, and falling perpendicular on the meridian of Paris.

According to the Advertiſement in the Map of France, the French computations have been made with a degree containing 57060 toiſes (=60811.7 fathoms); therefore, if we reduce

3° 35′ 42″.3 in the proportion of 61225 to 60811.7, we have 3° 34′ 14″.9 for the value of that arc when 61225 fathoms is = 1°; hence cosine 48° 34′ 35″ : rad. :: sine 3° 34′ 14″.9 : sine 5° 24′ 6″, the other longitude of Strasbourg. By the latter method the longitude of Cordouan was computed; but the other longitudes according to the former. The distances from the meridian of Paris are to be found in the publications alluded to in the above page.

P. 232. In the Table of General Results, *for* 1° 8′ 9″ and 4 m. 32 s. 36th. *put* 1° 8′ 4″ and 4 m. 32 s. 16th. the longitude of Padlesworth.

Against Calais, *for* 7 m. 23 s. 15.8th. *put* 7 m. 23 s. 15.2th.

Against Fairlight Down, in the last column but two, *for* 539.5 *put* 593.5.

P. 239. l. 15. *for* fig. 12. *put* fig. 13.

P. 242. l. 10. from bottom, *for* 3′ 55″ *put* fig. 3′ 35″.

P. 243. l. 13. *for* OKT *put* OK*t*. l. 5. from bottom, *for* *kt* put *kL*.

P. 224. l. 11. *for* $7''\frac{1}{4}$ *put* $7\frac{1}{4}$.

In the Table facing p. 246. in the column of *mean refraction*, for 0′ 15″.4 *put* 1′ 28″.1.

In addition to the examples of refraction in Sect. VII. the following (which was overlooked when this part was drawn up) may not improperly be given, as being of a different kind. It shews, that terrestrial refraction (though often much *greater*) must, at particular times, be much *less* than is generally supposed.

Oct. 7, 1787, at the station near Padlesworth, the depression of the horizon of the sea, in a SW direction nearly, was observed 26′ 27″. A degree of a great circle in this direction is about 61000 fathoms, and therefore $61000 \times 6 \times 57.2957795$

= 20970255 feet, will be the radius of curvature nearly. The height of the ſtation above low-water ſpring-tides (as determined by alternate obſervations at this place and Dover Caſtle) was 642 feet; hence $\frac{20970255}{20970255+642}$ = ,999969386 1 the natural coſine of 26′ 54″ the *dip*; therefore 26′ 54″ − 26′ 27″ = 27″ is what the horizon was elevated by refraction. The ſtate of the tide however, is not taken into conſideration; but the time was about noon.

The weather was calm and cloudy, and the horizon clear. Barometer 29.6. Thermometer 70°, at 1 P.M.

The above obſervation was made with great care and attention.

P. 249. in the 3. triangle, *for* 76807.5 *put* 76812.4.

The operations alluded to in the note at the botttom of this page were,

		°	′	″
Angles obſerved at Spring-Grove Houſe between	Richmond Ob. and the Pagoda	13	10	13
	The Pagoda and St. Paul's	23	17	15
	St Paul's and Harrow Spire	74	18	43
Angles taken on Fulham Church between	Stretham Church and St. Paul's	70	24	52
	St. Paul's and Hampſtead Church	44	50	46
	Hamp. Ch. and Hanger H. Tower	57	27	43

In 1787, at Hanger Hill, the angle between Richmond Obſervatory and the Pagoda was obſerved 12° 26′ 42″. At the Hundred Acres, that between Hanger Hill and Batterſea Church 23° 59′ 44″; and that between Hanger Hill and Stretham Church 42° 3′ 57″. The angle at St. Paul's between Batterſea Church and the SW Pinnacle of Weſtminſter Abbey is 9′ 45″.6.

The reſults from the obſervations made on Fulham Church cannot be conſidered as very exact, becauſe Hanger Hill Tower

itſelf was the object, inſtead of the Flagſtaff placed on it in 1787.

P. 250. in the 10. triangle, *for* 39963 *put* 48964.

In the 14. triangle, *for* 34413.6 *put* 34412.8.

P. 251. in the 20. triangle, *for* 39° 17′ 6″.5 *put* 39° 17′ 16″.5.

In the 27. triangle, there is a tranſpoſition of the two firſt angles, that oppoſite Fairlight ſhould ſtand oppoſite Goudhurſt.

P. 252. in the 28. triangle, *for* 5384 *put* 5385.

In the 35. triangle, *for* 23081.4 *put* 23018.4.

P. 254. in the 4. triangle, there is a tranſpoſition of the diſtances, 7198.2 ſhould ſtand oppoſite Hornſey Hill.

P. 255. in the 16. triangle, *for* 14390.2 and 6073.8 *put* 14390.8 and 6074.

P. 256. in the 22. triangle, *for* 8136.4 and 17641 *put* 8136 and 17640.3.

Neither of the angles of the 26. triangle was obſerved, becauſe St. Paul's is not ſeen from Greenwich Obſervatory. The diſtance of St. Paul's from Greenwich Obſervatory is alſo omitted. This diſtance is had from the VIII triangle, p. 173. and the 11. triangle, p. 250; from theſe we have,

		°	′	″	
the angle at	Norwood	42	15	26.5	hence Greenwich Ob. from St. Paul's 25655½ feet.
	Greenwich Ob.	82	41	1.1	
	St. Paul's	55	3	32.4	

The angles were determined thus:

	°	′	″	
Severndroog Caſtle	22	20	46	obſerved angles.
Greenwich Obſervatory	134	16	31	
Limehouſe Church	33	22	43	

Hence Greenwich Obſervatory from Limehouſe Church is 13999 feet.

Taking the ſum of 111° 56′ 50″ (in the VIII triangle, p. 173.) 82° 41′ 1″ and 134° 16′ 31″ from 360° there remains 31° 5′ 38″ for the angle at Greenwich Obſervatory between Limehouſe Church and St. Paul's; this, with the including ſides 13999 and 25655 (neglecting the fractions) will give 121° 1′ 42″ and 27° 52′ 40″, the other two angles.

P. 257. in the 31. triangle, *for* 6511 *put* 6925.4.

P. 259. The bearing of Greenwich from the meridian of St. Paul's, on which the other bearings in the Table depend, was found as follows:

	°	′	″
Angle at Greenwich Obſervatory between its meridian and Norwood .	38	7	16
between Norwood and St. Paul's	82	41	1.1
Hence the angle at Greenwich Obſervatory between the north meridian and St. Paul's .	59	11	42.9

This laſt angle, and its complement, with 25655½ feet the diſtance of Greenwich Obſervatory and St. Paul's, give 13138.5 for what St. Paul's is north from Greenwich, and 22036 for what it is weſt from the meridian.

On M. Bouguer's ſpheroid 13138.5 feet anſwer to 2′ 9″.5 on the meridian; hence the latitude of the point on the meridian of Greenwich, where a great circle, paſſing through St. Paul's, falls perpendicular on that meridian, will be 51° 30′ 49″½: and taking 61251 fathoms for a degree perpendicular to the meridian (ſee Tab. p. 227.) we have (22036 feet) 3′ 35″.86 for the intercepted arc of that circle; this, and the complement of 51° 30′ 49″½ (as the legs of a ſpherical triangle) give 89° 55′ 29″ for the angle at St. Paul's, which,

added

added to 30° 48′ 17″ (the complement of 59° 11′ 43″) gives 120° 43′ 46″, for the bearing of Greenwich Obſervatory, as in the Table.

P. 259. againſt St. Luke's, *for* 12° 57′ 53″.7 *put* 12° 37′ 37″.

Againſt Shoreditch Church, *for* 44° 57′ 39″.8 and 6746.4 *put* 44° 54′ 58″.8 and 6743.2.

Againſt Severndroog Caſtle, *for* 115° 28′ 50″.4 *put* 115° 25′ 50″.4.

Againſt Eltham Church, *for* 123° 46′ 4″.2 *put* 123° 46′ 4″.

P. 260. againſt Stretham Church, *for* 31793.5 *put* 31739.5.

Againſt Clapham Common, *for* 26° 29′ 56″.1 *put* 26°29′ 52″.

Againſt St. Bride's Church, *for* 1771.7 *put* 1687.6.

Againſt St. George's Bloomſbury, *for* 103° 15′ 20″.6 *put* 103° 15′ 50″.

Againſt the Tabernacle, *for* 107° 20′ 47″ *put* 107° 19′ 47″.

Againſt Highbury Houſe, *for* 178° 43′ 11″.6 *put* 178° 43′ 14″.6.

In Plate X. fig. 2. *p* is omitted at the concourſe of the meridians *eb*, *na*. Alſo a line from A to T in fig. 3. ib. *for* F *put* R.

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AN

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TO THE

EIGHTIETH VOLUME

OF THE

PHILOSOPHICAL TRANSACTIONS.

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